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ADDRESS IN PHYSIOLOGY.

BY

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MR. PRESIDENT AND GENTLEMEN,—When the committee of your Association did me the honour to request me to deliver an Address in Physiology at this meeting, I acceded to their wish; not, I must confess, without some feelings of my own incompetence, and with a strong conviction of the difficulties of the task I undertook. When I began to reflect on the subject, it seemed to present difficulties of no ordinary character, from the great abundance of material it afforded. I felt that to attempt to generalize on the science of physiology, in the short space of time allotted to an address, would tax powers far greater than mine; and that to endeavour to sketch the recent advances which the various cultivators of the science, in its different departments, had made, would necessarily be attended with failure.

It is true, that some of the great principles which regulate the phenomena of life admit of easy definition, and great and varied application; and no more interesting and important subject could possibly be chosen as the groundwork of an address: but yet, when we come to deal with many of the questions of the science, we often find ourselves rapidly passing from the domain of well grounded theory and recognised laws to that of hypothesis; and from our incomplete knowledge of many of the manifestations of vital action, we are compelled to rest satisfied with but an imperfect explanation of the facts we witness.

But, in making this remark, it is impossible not to rejoice at the high position which physiology now holds as a science, and at the great advances and brilliant discoveries which have marked its progress during a period comparatively recent; discoveries which have almost revolutionized the whole science of organic life—which have thrown a flood of light on the highly complex phenomena of vitality, and reduced many of them to the utmost simplicity; discoveries which have resulted in generalizations of an application so universal, and an importance so vast, as to entitle them to no lower position as established truths than that accorded to the doctrines of physical or chemical science; for what are the generalizations in physics, or what the laws of chemistry, which have a wider range, or an application more universal, than the principles of nervous action enunciated by the genius of Bell, or of cell-formation, mainly elucidated by the labours of Schwann?

To attempt to sketch the recent progress of physiology in all its varied departments would be alike impossible with an attempt at generalization, so great have been the labours of the cultivators of the science, so numerous are the details of their observations.

Unwilling, therefore, to venture on the responsible task of addressing you on the general principles of the science, and feeling the impossibility of reviewing the whole mass of the subject, I resolved to deal with it in one of its details; a determination to which I the more readily arrived, that I felt that, in addressing those who are engaged in the practical duties of their profession, I should best discharge my functions by giving the subject, as far as I was able, as practical a bearing as possible.

Following, therefore, the example of others who have delivered addresses on kindred subjects, I have felt that in selecting some topic to which more especially to direct your attention, I should be acting quite in accordance with the wishes of my audience, and with the requirements of the subject itself. Indeed, such a course is rendered absolutely necessary at the present day, for to whatever branch of science we direct our attention, we find its sub-divisions so numerous and important, that each one claims a special share of our attention.

But when, after much careful consideration, I determined to choose some special subject as the chief topic of this address, and had flattered myself that I had surmounted an important obstacle, I experienced no small difficulty, and no little trouble, in deciding which branch of the science I should select—on what series of phenomena I should venture to offer you a few observations.

On considering the various subjects which have engaged the attention of physiologists in recent years, it appeared to me that there was one which, from its importance, the labours devoted to it, and the great advances made in it, possessed undisputed claims to consideration; and, as I am not aware that it has ever been made the subject of a special address in connection with this Association, I have felt that it could not fairly be passed over.

There are no departments of physiology which possess greater interest, there are none in which there have been greater labours, as assuredly there are none which present greater difficulties, than those which relate to the investigation of the functions of the *Nervous System*; and undoubted as have been the advances which have taken place in our knowledge of the actions of the various portions of the nervous centres, during the present century especially, yet that knowledge, we are compelled to confess, is still in many respects imperfect and incomplete; and there is no subject in the whole range of organic science which opens a wider field for research, no one which promises a more abundant harvest with reference to the principles of diagnosis and treatment of disease, than that which relates to the phenomena of nervous action.

My object, to-day, will be to lay before you some observations on the functions of some portions of the nervous centres, and especially to direct your attention to the more recent and very important facts, which experimental physiology and pathological observation have brought to light with reference to the properties of the spinal cord.

The investigation of the properties of the nervous centres is undoubtedly surrounded with immense difficulty. That important clue to function which the knowledge of the minute structure of organs affords, has been hitherto in great measure denied us with reference to the nervous centres. The peculiar nature of the nervous tissue has prevented us from ascertaining exactly how certain portions are disposed of; and although some researches, to which I shall have to refer, have thrown great light on questions of this kind, and have received confirmation from subsequent physiological observations, yet there is still wanting that absolute anatomical proof, without which, as a rule, no safe theory can be built up with reference to function. But were our knowledge of the structure and arrangement of the nervous system perfect; did we know the exact mechanism by which the phenomena of nervous action are performed; were we acquainted with the processes by which nervous power is generated; or of the nature of that subtle fluid which conveys the mandates of the will, and carries impressions to the sensorial centres—there would yet remain endless subjects for inquiry? a vast mass of facts in connection with that system in which mind is so intimately blended with matter, which is the organ of the intellectual faculties, the source of man's supremacy over the rest of the organic world.

It would be interesting, did time permit, to dwell on the views which were entertained by the ancients with respect to

the subject we are considering. These views, however, almost entirely speculative in their character, have but little practical bearing. It was long an opinion, which seems to have been promulgated by Aristotle, that the brain was of no use except as a refrigerator of the hot spirits ascending from the heart, whilst we find that Plato, Galen, and, in a later age, Vesalius, considered it as the seat of the rational faculty of the mind. At an early period, a glimmering of the truth seems to have existed with reference to the functions of the different nerves; for we find Erasistratus teaching that the sensory and motor nerves had different origins; the former arising from the meninges, the latter from the cerebrum; whilst Galen, who performed some interesting experiments on the spinal cord, experiments which have been recently repeated with the most important results, considered that the soft nerves were nerves of sensation, and the hard, nerves of motion. Galen opposed the views of Aristotle with reference to the functions of the brain, the ventricles of which he considered were for the purpose of secreting the animal spirits, to be then diffused throughout the substance of the brain, and from thence transmitted to the nerves; and this opinion, with some modifications, was long entertained. By the Arabs, one of the ventricles was supposed to be the seat of sensation; another, of the imaginative faculty; a third, of the understanding; and a fourth, of memory; and Benevenius, in confirmation of this doctrine, relates the case of a certain thief, often caught stealing, who never could recollect his previous offences, and who was found after death to have no cerebellum.

Numerous authors, who flourished after the revival of learning, maintained more or less fully the opinions of Galen. Descartes asserted that the animal spirits were secreted from the brain through pores opening into the ventricles, and that when accumulated there they excited the soul through the pineal gland, from which they were distributed to the body. Caspar Bauhin was amongst the first who taught that the animal spirits were generated in the substance of the brain, and from thence were distributed to the nerves, without being collected in the ventricles, and in this view he was supported by Caspar Hoffman. These authors also taught that the ventricles were excretory organs, and their secretion was supposed to find its way through the cribriform plate into the nostrils and fauces, and thus to produce the mucus of those cavities; an opinion which it was reserved for the anatomical researches of Schneider to refute. A view which later prevailed, and which was supported by our illustrious Willis, by Malpighi, and others, was that the cortical substance of the brain was the generator of the animal spirits; a doctrine analogous to that which is conformable with all we know at the present day, with reference to the grey matter of the nervous centres as the generator of nervous power.

But I feel that I must pass from these considerations to more practical points, and more recent observations in connection with my subject.

Valuable as may have been the opinions, and true as may been the theories, of some of the distinguished physiologists who flourished during the eighteenth century, especially with regard to the functions of the spinal cord, a point on which I shall have briefly to dwell, it may be safely said that with reference to our knowledge of the functions of the roots of the spinal nerves, we owe everything to the labours of more recent investigators. Before the time of Sir Charles Bell, all was hypothetical and vague. His discovery at once placed the science of neurology on a firm and stable basis; its importance, its value, the influence it has exercised on subsequent investigations, it is impossible to overestimate. A type of all great discoveries, the result of careful observation and profound reasoning, it stands preeminent amongst the results of the labours of the present age, and marks an epoch in the history of physiological science.

It would be pleasing to dwell in the language of eulogy on the merits of him to whom science is so much indebted, and briefly to sketch the progressive steps by which he was led to his important results; to show how he saw with his mental eye the structure of those cords, the function of which he had discovered; and how, whilst unable to see that which the microscope has since demonstrated—the isolation of each nervous fibril—he yet was led to appreciate accurately the important fact—but time forbids, and I pass to other topics.

There is no subject which presents peculiarities of greater interest, there is, perhaps, no one which has led to greater controversy, in connection with the nervous system, than that which relates to the reflex, or automatic, movements of the body; and whilst it will be admitted by all that to the labours of one of our most distinguished countrymen we are mainly indebted for a more definite and accurate knowledge of these actions than formerly existed, yet we feel that justice would not be done to others, did we pass over in silence the views and the labours of physiologists of a past century, who seem to have appreciated in a very remarkable manner the nature of the phenomena to which we are alluding.

Those who have read the works of Whytt, of Unzer, and of Prochaska, cannot but have been convinced that those authors had formed very accurate views of the various involuntary movements of the body; and, although some discussion has taken place as to the exact meaning of a term used by Whytt, yet there can be but little doubt that he appreciated the fact, that an impression made on the periphery of a nerve might be carried to the nervous centre, and there, without producing sensation, give rise to a reflex action. But Unzer has enunciated, in a much more complete manner than Whytt, the doctrines of reflex action; and has clearly pointed out the distinction between acts resulting from impressions only, and true psychical actions derived from the brain. He says motions may be produced as the result of external impressions, although these impressions cannot reach the brain, even after an animal has been decapitated, so that no sensations can be felt. He calls them "nerve actions," in contradistinction to those which are the result of consciousness, which he terms "soul actions." I cannot give the quotations from Unzer's work which shew clearly his views, but he distinctly enunciated the principle, that the nervous system is not simply destined to propagate external impressions to the brain, there to produce sensation with consciousness; but that it possesses also the distinct property of giving rise to movements as the result of an external impression, which does not reach the organ of consciousness; and, in an answer to one of his reviewers, he distinctly expresses his belief in the reflex function of the spinal cord, for he says that, as nerves connected with ganglia may experience from external stimuli a *moving power*, a reflection of impressions upon other fibres which are regularly determined to action, "*just so is it with the nerves which arise in the spinal cord, and have there probably the double function: partly to transmit these impressions to the brain, that sensations may be excited, and partly, to be reflected on other nerves, so that thereby certain movements are developed which otherwise would not result from these impressions.*"

It is impossible to enter fully into the views of Unzer, many of which have received confirmation at the hands of physiologists of recent date; but I must not pass over the works of one of his successors, Prochaska, who flourished at the close of the last and the beginning of the present century, and who more accurately defined than previous physiologists the portion of the nervous centres from which reflex actions are derived. After stating that external impressions made on sensorial nerves are transmitted to the origin of the nerves, and are thence reflected on to corresponding motor nerves, he says that the part where these sensorial and motor nerves meet, *and whence motor actions are reflected*, is called the *sensorium commune*. This sensorium commune consists, not of the cerebrum proper and cerebellum, but extends through the medulla spinalis, the medulla oblongata, the crura of the cerebrum and cerebellum, and part of the thalami optici; in a word, he says it is co-extensive with the origin of the nerves. He was fully aware that reflex actions might take place independently of consciousness, and he terms them automatic actions. He is also, I believe, the first author who mentions that these reflex actions take place in obedience to a law which regulates the preservation of the individual.

These observations are sufficient to show, that the various phenomena of reflex action were well known to the physiologists I have alluded to, and that they had analysed their relations to the nervous system with great sagacity.

It has been a common impression that, up to the time of Marshall Hall, these various so-called sympathetic actions were regarded by physiologists as dependent on sensation; for, to use the words of one of the writers in the *British and Foreign Medical Review* for 1837, "The phenomena called excito-motory are precisely those which come under the old denomination of sympathetic; and which we believe have generally been thought to be dependent on sensation." Now it would not be difficult to show that, although the views of Unzer and Prochaska seem to have been but little received, there is yet abundant evidence that all physiologists did not consider these movements as dependent on sensation. Time does not permit that I should refer to the passages in the writings of such authors, in proof of this statement; but the

works of Hunter, of Sir Gilbert Blane, of Mayo, of Flourens, will furnish abundant instances.

But notwithstanding this, it must be admitted that a very imperfect notion generally prevailed of the nature of these automatic movements; and Dr. Marshall Hall was undoubtedly the first to revive the doctrines of reflex action, to establish, with greater precision than his predecessors, the laws by which its phenomena are governed, and to assign to them their proper position in physiological science. Although he was to a certain extent anticipated in some of the essential parts of his system, yet he was the first to apply that system to many important pathological phenomena, and to the elucidation of various forms of nervous affections. The physiology and pathology of the nervous system received a new impulse from his researches. His whole life was devoted to the study and investigation of the reflex or excito-motor system, which he pursued with all the perseverance and ardour which characterize the discoverer, fully convinced of the importance of his doctrines. He worked incessantly to illustrate his principles, to simplify and enlarge the basis on which they were founded; and whatever may have been the deficiencies or the imperfections of his system, the prominence he gave to his views, by the number and scientific character of his writings, and his own indefatigable exertions, had the effect of directing the attention of observers in all parts of the world to the facts of which he treated, and of establishing, as a general principle of science, that which had been previously admitted only by a few.

The special views of this distinguished physiologist, that there is a distinct set of excitor nerves and motor nerves belonging to the reflex system, independently of the ordinary sensory and voluntary nerves, as yet rests on no absolute anatomical proof, and has never been universally admitted as true. That each portion of the spinal marrow is an independent centre of nervous action, admits of indubitable proof; and that the cord is the channel of communication with the organ of consciousness, I need scarcely mention. The exact mechanism by which the automatic movements are accomplished, and by which sensitive impressions are conveyed to the brain, and motor impulses to the muscles, yet requires elucidation; but there are some facts which examination of structure, and experimental investigation, have brought to light, that have important bearings on this point, and on some of these I propose briefly to dwell.

By most anatomists, before the time of Gall and Spurzheim, the spinal cord was considered as a mere bundle of nerves, a prolongation from the brain, but those authors pointed out the untenable nature of this view. The existence of the cord in acephalous fœtuses, its size in proportion to the number of nerves derived from it, and the direction of the fibres of the nerves, led them to maintain this opinion. They further stated that the nerves arose from the grey matter of the cord, but the statement seems to have been founded on no anatomical proof. The first anatomist in this country, who appears to have investigated the relation of the roots of the nerves with the spinal cord, was Mr. Grainger; and his observations showed that these roots were in part connected with the grey matter, and he was led to believe that they were in part continuous with the longitudinal fibres of the cord, and thus passed on to the encephalon. The observations of Mr. Grainger were somewhat anticipated by those of Bellingeri, who had previously traced the anterior roots to the grey matter in the ox.

Since the time when Grainger made his observations, numerous anatomists have directed their attention to the structure of the spinal cord and the arrangement of the roots of the nerves; and, by employing various processes of preparation, have been able to make thin sections of the cord, such as they could examine with tolerably high powers under the microscope. Amongst the most important observations that have been made are those of Volkmann, Kölliker, Stilling, Lockhart Clarke, Schilling, Gratiolet, and most recently of all, Lenhossèk and Schroeder Van der Kolk. Considerable discrepancy exists between the statements of these authors. On the one hand, we find Volkmann asserting that the longitudinal columns of the cord undergo no progressive increase of size from below upwards. Now this is a very important point; for if the sensory and voluntary nerves are derived from the encephalon, and pass off from the cord in the various spinal nerves, the cord must contain a much larger number of fibres at its upper than at its lower part, and we should expect that the columns of the cord would gradually increase in size from below upwards. Volkmann further maintains that the quantity of white matter is everywhere in proportion to the quantity of grey; his observations were made on the spinal cord of the horse, and have been contradicted by Kölliker, who asserts that in man the thickness of the white columns does augment from below upwards, and that the increase in the diameter of the cord, at the ganglionic enlargements, is due to the augmentation of grey matter only. He, moreover, states that the nerve-tubes of the cord, especially in the upper part, are much smaller than those of the nerve-roots; and he asserts from actual measurement, that it is by no means impossible for the fibrous strands of the cord to contain all the nerve-tubes of the spinal nerves.

The researches of Mr. Newport into the structure of the articulata, seemed to establish a very important fact in connection with the nervous system of those animals. He stated that he had found that certain of the fibres of the nerves proceeding to the analogue of the spinal cord, passed onwards to the cephalic ganglia, whilst others passed into the ganglia of the cord.

The theory deduced from Newport's view of the structure of the nervous system, is that the nerves which proceed to the cephalic ganglia, are the agents by which sensations are conveyed to the latter, and in respondence, these ganglia direct and harmonise the general movements of the body; whilst the nerves which are simply connected with the ventral ganglia are the agents of the excito-motor actions.

This view of the structure and functions of the spinal cord has been adopted by Dr. Carpenter, who considers that the cephalic ganglia of the invertebrata are analogous to those ganglia which are found at the base of the brain in vertebrata, and which he believes to be the centres of sensation and instinctive motor action, whilst each portion of the spinal cord ministers to the automatic movements of the body. Dr. Carpenter endeavours to reconcile the structural views of Kölliker and Volkmann, on the principle that it is not at all improbable, that different animals should have different proportions of nerve-fibres terminating in their automatic and sensorial centres, according as their movements are more or less analogous to those of the invertebrata, or more or less performed in obedience to the influence of the will, and under the guidance of sensation.

Notwithstanding the high authority on which these views rest, they are not conformable with the results obtained by the observations of other investigators. From microscopical examinations of the disposition of the roots of the nerves, there can be no doubt that many of the fibres terminate in the grey matter of the cord, at or near the spot where they join it. Lockhart Clarke has shown that the posterior roots, on entering the cord, are disposed of in three ways; some of their fibres pass transversely, and enter the grey substance; others pass downwards for a short distance, and then terminate in a similar manner; whilst a third set pass obliquely upwards, but he has not been able to trace any of them continuous with the longitudinal columns of the cord, and the inference therefore is that, after a longer or shorter course, they enter the grey substance. This view of the disposition of the fibres of these roots is borne out in a remarkable manner by experiments on living animals. With regard to the anterior roots, they have been traced to the anterior cornua, and they are said to take their origin from ganglion cells, which exist in groups along the cornua. This arrangement of the anterior roots has been most recently pointed out by Schroeder Van der Kolk, who does not admit that any of their fibres are directly continued on to the brain; he differs, however, from Lockhart Clarke and others with regard to the fibres of the posterior roots, which he says consist in part of sensitive nerves, and in part of excitor nerves; the former passing up in the posterior columns to the encephalon, whilst the latter are connected with the grey matter of the cord. In support of this view, he states that the anterior columns do not increase in size from below upwards, but that the posterior columns progressively enlarge.

These views of Schroeder Van der Kolk, and the physiological inferences he deduces from them, are opposed, in a very important manner, to the results of experimental investigation into the functions of the columns of the cord, to a brief consideration of which I now pass.

Before the time of Sir Charles Bell, when the difference in function of the two roots of the spinal nerves was unknown, no attempt had been made to assign different functions to different columns of the cord. Sir C. Bell first promulgated the view that the posterior columns, like the posterior roots of the nerves, were the conductors of sensitive impressions, whilst the antero-lateral were the channels of motor influence; he, however, gave no further support to his theory than the following very incomplete experiment:—"I found," he says, "that injury to the an-

terior portions of the spinal marrow convulsed the animal more certainly than injury to the posterior portion; but I found it difficult to make the experiment without injuring both portions."

This theory of Bell, which I may here state was subsequently in great measure abandoned by him, was opposed by Bellingeri, Schœps, Rolando, Calmeil, Nasse, and others; but was strongly supported by an eminent French physiologist, M. Longet, who attempted to prove that it was as correct as that respecting the functions of the roots of the nerves.

As the views of M. Longet have been extensively received as true, both by physiologists in this country and on the Continent, and as they seem to me, especially with reference to the channels for the conduction of sensitive impressions, to have received an entire refutation at the hands of recent investigators, I feel it my duty to consider them somewhat in detail.

M. Longet is of opinion that all the sensitive nerve-fibres of the spinal nerves enter the posterior columns of the cord, and go up to the brain in these columns, and that therefore sensitive impressions are only received from these parts of the cord and their prolongations in the encephalon:

That the restiform bodies of the medulla oblongata, being the continuations of the posterior columns of the cord, are also the only channels for the transmission of sensitive impressions:

That the sensitive impressions, going to the sensorium, have to pass chiefly across the cerebellum, as the restiform bodies chiefly pass across that organ.

Dr. Todd, in this country, raised the first serious objection to this view. He pointed out, on anatomical grounds, the improbability of the theory, and detailed some pathological facts which tended to negative it.

With the exception of the researches of Schroeder Van der Kolk, recent anatomical evidence is opposed to the theory; but the most important evidence which has yet been given with reference to the question, is to be found in the results of the experimental observations and in the pathological facts detailed by Dr. Brown-Séquard.

The experiments of Dr. Brown-Séquard were made on some of the lower mammalia. They consist in laying bare a portion of the spinal cord by removing the arches of the vertebræ, and in dividing different portions of the cord. We are assured by the experimenter that the objections which have been raised against his views, on the ground that no satisfactory results can be obtained after the spinal canal is opened, are altogether untenable; for, "When the operation is done quickly, even if a considerable part of the cord be laid bare, if hæmorrhage have not been great, and if pain have been avoided by the exhibition of chloroform, there is no notable diminution of sensibility, and there is no diminution in the voluntary movements, except that depending upon the section of the muscles of the back."

The first fact pointed out by Dr. Brown-Séquard to which I shall refer, is that a transversal section of the posterior columns of the cord does not produce a loss, nor even a diminution, of sensibility; on the contrary, that it is followed by an increase in the degree of that property. The section, therefore, of the supposed only channels of transmission of sensitive impressions results, not in anæsthesia, but in hyperæsthesia in all the parts supplied with nerves behind the seat of section. Further, it appears that a complete transverse section of the restiform bodies is followed by an increase of sensibility in every part of the limbs and trunk; and that the same results, but in a less degree, follow a transversal incision into the cerebellum, the *processus cerebelli ad testes*, and the *tubercula quadrigemina;* and that if the restiform bodies be dissected from their neighbouring parts and removed, the animal operated on becomes hyperæsthetic.

These are assuredly very important facts, and they afford convincing proof, at any rate, that the posterior columns of the cord, and the restiform bodies, are not the *only* channels by which sensitive impressions are conveyed from the posterior roots of the nerves to the encephalon; but other experiments tend to show that these parts are in no way concerned in the function at all.

After the experiment I have detailed, the natural step to take is to ascertain the result of division of all the parts of the cord except the posterior columns. Such an experiment has been frequently performed. Without mentioning the results obtained by Stilling, Schiff, and others, I may state that Brown-Séquard has found that when the whole of the spinal cord, except the two posterior columns, is divided transversely, say, for instance, in the dorsal region, the posterior limbs become deprived of sensibility. This experiment affords another proof that the posterior columns are not the channels for the transmission of sensitive impressions to the brain, and other observations seem to show that the lateral columns of the cord have no part in the function.

A variety of experiments it is unnecessary to detail, which consist in dividing every part of the spinal cord except the central grey matter, tend to prove that this central grey matter is the principal conductor of sensitive impressions to the brain; but there are some facts which indicate that it is not the only portion of the cord thus employed, and that the anterior columns have a certain share in the function, for it has been found that, when the whole of the grey matter is divided, the anterior columns being left, sensibility, which disappears immediately after the operation, after a time returns to a slight extent.

A very important question arises, as to what part of the grey matter is the conductor of these sensitive impressions. Experiments seem to show that the central portion, that connecting the cornua together, is the most concerned in the function. We learn from the researches of Lockhart Clarke, Schroeder Van der Kolk, and others, that the grey matter consists in part of ganglion-cells, and in part of nerve-fibres, the latter of which form longitudinal columns, surrounded by grey substance. No proof, however, exists that they pass continuously up to the brain, and Lenhossèk maintains that they are communication-fibres between different ganglion-cells. If any of the fibres are continued on to the brain, they probably take part in the function of transmitting sensitive impressions. Although the posterior columns of the cord appear to have no share in transmitting sensitive impressions to the encephalon, yet there is proof that the fibres of the posterior roots pass through them to enter the grey substance; for, when a transverse section of these columns is made, the nerve-roots attached to the inferior segment are highly sensitive, a fact which seems to be due to some of the fibres of those roots passing downwards to reach the grey matter. This experimental observation entirely accords with Lockhart Clarke's anatomical researches previously alluded to.

The general conclusions which may be deduced from experiments and observations which I cannot detail, are that the conductors of sensitive impressions, when they reach the spinal cord, either at once terminate in the grey matter, or else, passing obliquely for a short distance, some upwards, and some downwards, ultimately terminate therein.

Another important question, which seems not to have occupied much of the attention of physiologists, but which appears entirely set at rest by recent experiments, is that which relates to the decussation of the conductors of sensitive impressions in the spinal cord.

It was the universally received opinion that no such decussation took place, and therefore that the sensitive impressions must cross in the encephalon. Sir Charles Bell tried to discover the seat of decussation, and he imagined it to be above or near the crossing of the pyramids; but he gave no proof in support of his idea.

The section of a lateral half of the spinal cord is the experiment most calculated to give us proof of the decussation, or otherwise; and such an experiment has been frequently performed; but unfortunately no observations, until recently, were made as to its effects on the sensibility of parts supplied with nerves behind the section. Schœps Van Deen and Stilling found that sensibility was not destroyed on the side of section; but they did not recognise the condition of the opposite side.

We are indebted to Brown-Séquard for the solution of the question we are now considering, and for the discovery of the fact that there is a decussation in the spinal cord of the conductors of sensitive impressions. The following are the results of his observations. He has found in mammals that, when a section of a lateral half of the cord, including the posterior, the lateral, and the anterior columns, with all the grey matter of one side, is made, sensibility seems much increased on the side of section, and much diminished or entirely lost on the opposite side. In fact, there is hyperæsthesia on the side on which the division is made, anæsthesia on the other.

But another experiment may be performed to prove that decussation takes place. It is one which was performed by Galen, and was repeated by Flourens; but its effects on sensibility were entirely overlooked by both. It consists in making a careful division of the cord—say of the lumbar portion—into two lateral halves. If such an experiment be well performed, only the commissures of the cord are divided, and all the longitudinal elements are left uninjured. A striking result is then

obtained: the voluntary movements still exist in the posterior limbs, but sensibility is entirely lost in them. This loss of sensibility must depend on the section of the sensitive conductors, as they cross the median plan of the cord.

From a vast number and great variety of experiments, to the details of which I must refer all who desire to investigate this subject, it appears that we may consider it as an established fact, that the conductors of sensitive impressions make their decussation in the spinal cord, at a short distance from the point of insertion of the posterior roots of the nerves. Experimental investigation seems to show that this decussation is complete in the lower mammalia, and pathological facts appear to confirm the view with regard to man.

There are certain pathological phenomena of great importance, hitherto difficult of explanation, on which much light has been thrown by recent experiments. It is well known that considerable alteration of the spinal cord may exist without loss of sensibility ensuing. It appears that, when the spinal cord is divided transversely, and the section reaches the grey matter and divides a portion of it, sensibility becomes diminished in every part of the body behind the seat of section, and not more so in one than in another; and a partial section of one side is followed by diminution of sensibility on the opposite. From these results, it would appear that, however small be the portion of grey matter remaining, it is capable of transmitting a certain amount of sensation from all parts of the body behind it. We here have an explanation of the fact so often seen in cases of disease of the spinal cord—a persistence of sensibility, when, after death, we find a large portion of the cord destroyed.

From the consideration of the parts in the spinal cord, which are the agents for transmitting the sensitive impressions to the brain, we naturally pass to those concerned in the transmission of the orders of the will to muscles.

The theory of Sir Charles Bell, that the anterior columns were destined for this function, rests on no proof afforded by that distinguished physiologist; but many experimenters have stated that a section of these columns produces paralysis. Stilling has, however, denied this. The experiments of Brown-Séquard tend to show that a section of the posterior columns does not affect the voluntary movements, but that a division of the whole of the cord except the posterior columns produces complete paralysis. He further states, that a section of the posterior half of the cord, including the whole of the central grey matter, is followed by an almost total annihilation of voluntary movement; and if the anterior horns of the grey matter be divided as well, the loss of power seems complete. This would lead us to infer, that the anterior columns have but little or nothing to do in the transmission of the orders of the will to muscles; but then, on the other hand, it is found that a section of these columns alone is followed by an almost entire loss of power. From these observations, we are forced to the conclusion that all parts of the cord, except the posterior columns, are more or less concerned in transmitting the orders of the will; but this remark does not apply to the upper part of the cord, for it appears that, near the medulla oblongata, most of the voluntary conductors pass in the lateral columns and the grey matter between the lateral and anterior columns.

The results I have mentioned, as following the section of the anterior columns of the cord, in some measure agree with the views, founded an anatomical grounds, of Schroeder Van der Kolk, who is of opinion that the fibres of these columns terminate in the ganglion-cells of the grey matter, from which the motor nerves arise; and that they transmit the influence of the will to the ganglion-cells, which then act on the nerve-fibres.

If we compare the results obtained by vivisections of the lower animals with those which pathological changes in the spinal cord have produced in man, we find a remarkable confirmation of the theories which have been advanced. A large and very admirable selection of cases has been made by Brown-Séquard, which tend to throw much light on the functions of different portions of the cord. And first, with regard to the posterior columns, it has been found that, whether these columns are injured by a wound; whether they are submitted to pressure by a tumour or by a piece of bone; or whether their structure is to a slight extent destroyed,—the sensibility of the parts behind the seat of injury remains. Hyperæsthesia even has been known to exist, just as takes place in the lower animals after section of the columns.

These cases also prove that the grey matter is the chief channel for transmission of sensitive impressions. Several cases are recorded in which sensibility was persistent, where *post mortem* examination revealed extensive softening of the white substance, the grey matter being apparently in a normal state; and, on the other hand, cases are mentioned which show that destruction of the grey matter, the white substance being unimpaired, is attended with a diminution or loss of sensation.

Pathological facts afford abundant evidence of the decussation of the sensitive conductors in man. On this point, I shall content myself with remarking, that a careful examination of a considerable number of reported cases appears to prove that such a decussation takes place near the spot at which the fibres enter the cord; and that, having decussated, they pass upwards chiefly in the grey matter to the encephalon. Observation shows that no decussation of sensitive fibres takes place in the pons Varolii; all have decussated before reaching that spot. Disease, or injury, or pressure of the pons, medulla oblongata, or medulla spinalis, produces a loss or diminution of sensation on the side opposite the affection.

I have alluded to the circumstance that experiments seem to show that, at the upper part of the spinal cord, the anterior columns do not transmit the mandates of the will. Anatomy and pathological facts tend to confirm this. The decussating fibres of the anterior pyramids are almost entirely derived from the lateral columns of the cord; and thus there is no decussation of the anterior columns. Notwithstanding this, an alteration of one side of the medulla oblongata, above the decussation, produces paralysis of the opposite side; whilst an alteration of one side of the cord, below the decussation, produces paralysis of the same side.

Dr. Brown-Séquard has attempted to prove that the various kinds of sensitive impressions have different conductors in the spinal cord; and also that the nerve-fibres employed in transmitting to the cord the sensations of *touch*, *tickling*, *pain*, *heat*, and *cold*, and the sensation which accompanies *muscular contraction*, are as distinct one from the other as they all are from the fibres of the motor nerves. He mentions several cases in which some of the sensations were absent whilst others remained; where, for instance, there was loss of feeling of *tickling* or *contact*, persistence of feeling of *pain;* loss of *tactile sensibility*, of the faculty of feeling *pinching*, *pricking*, and the passage of a *galvanic current*, and persistence of power of feeling *cold* and *tickling*. These certainly are very remarkable cases; but, before we can admit the independence of the nervous fibres conveying these various sensations, very strong evidence is necessary. I would venture to submit, that it is not at all improbable that a single nervous fibre may have the power of transmitting the various kinds of sensations alluded to, depending upon the nature and the power of the stimulus by which it is excited, and the locality on which the stimulus acts.

And here I may, perhaps, be allowed to say a word with regard to those nerves which are supposed to be solely employed as the excitors of reflex action; the existence of which is maintained by Schroeder Van der Kolk on anatomical grounds. The great bulk of evidence which microscopical observations afford us, seems to me to show that the nerve-fibres of the roots of the spinal nerves terminate in the grey matter of the cord, and that none of them are continued on to the brain; and this view, which was originally promulgated by Dr. Todd, appears to receive confirmation from the experiments I have detailed. According to this view, the grey matter of the spinal cord receives the impression from the centripetal nerves, and either carries it to the encephalon, to produce there a sensation, or, if the communication with the encephalon be cut off from any cause, the impression does not result in sensation, but may give rise to a reflex movement, from the independent action of that part of the spinal cord which received the impression.

With respect to the existence of nerve-fibres for the special function of the excito-motor system, as I have previously stated, no absolute proof exists; and, although it may be somewhat contrary to our notions to believe that a nerve-fibre is capable of performing the double function, of conveying at one time a sensitive impression to the nervous centre, and at another, a stimulus which produces a reflex motor act: yet there seems to me to be nothing improbable in the view, for the impression made is the same, whether it result in a sensation or a motor action; the difference is in the condition of the nervous centres, which, in one instance, allow of its transmission to the sensorium, and in the other do not. The impression made on a centripetal nerve, and received by the cord, would become a sensation were there nothing to prevent its transmission to the brain; but this impediment existing, the impression results in a reflex act, which would have been controlled had the impression been felt.

A view has been advanced by a physiologist, to whose opinion

much attention is due, that the spinal cord is not only a centre of motor action, but also of sensation and volition; and that decapitated animals exhibit phenomena which are not explicable except on the supposition that sensations are perceived, and that these give rise to movements which are guided and controlled by the will. This view seems to me to be open to very serious objections; and all the phenomena to which Mr. Lewes has referred in support of his view, appear to me to be simply instances of the excito-motor action of the spinal cord.

Although it is now generally agreed amongst physiologists that the spinal cord is an independent centre of nervous action, and that one part can act without the other; yet it is still maintained by some that the reflex power, which the cord possesses after it has been separated from the encephalon, is the remainder of an influence derived from the encephalon before its separation. In opposition to this view, it has been shown that, in an animal in which the spinal cord is divided, and which has been subsequently killed by cutting the carotid artery, after the reflex action has ceased, it will return if blood be injected into the body so as to restore the circulation in the cord; thus showing that the reflex faculty is a vital property of the cord, depending on the organisation of the part. The reflex faculty often remains after considerable injury has been done to the spinal cord; for instance, when it has been crushed,—a point of some practical importance as tending to show that, although reflex action may exist after a fracture or luxation of the spine, the cord may be very far from sound.

[*To be continued.*]

Illustrations
OF
HOSPITAL PRACTICE:
METROPOLITAN AND PROVINCIAL.

KING'S COLLEGE HOSPITAL.

CASES OF ABSCESS TREATED BY MEANS OF CHASSAIGNAC'S DRAINAGE-TUBES.

Under the care of W. Bowman, Esq., F.R.S.

We are indebted to Mr. Atchley, house-surgeon to the Hospital, for the notes of the following cases, which are reported, not account of any peculiarity in themselves, or any brilliancy of success, but in order to bring before our readers a plan of treatment which is now extensively pursued, especially at Middlesex and King's College Hospitals, in cases of large chronic abscesses, whether depending on diseased bones or on other causes. The method is extremely simple, and is quite devoid of pain. It consists merely in passing into the abscess a tube of India-rubber, perforated with holes at intervals, and one-sixth or one-fourth of an inch in diameter. When a counter-opening can be established, it is, of course, an advantage; and then the tube is passed from the lower to the upper opening. The advantages of this plan in the treatment of empyema was recently insisted upon in a paper by Dr. Stewart and Mr. De Morgan, read at the Royal Medical and Chirurgical Society. In M. Chassaignac's practice, we understand that the formation of a counter-opening is considered as an essential part of the treatment; but in London this is not held to be indispensable, as it is found that, in abscess depending, for example, on disease of a deep-seated bone, the presence of an agent which ensures a direct communication between the diseased bone and the surface, without blocking up the wound or opposing any obstacle to the escape of the pus (which, on the contrary, it facilitates), exercises an important influence in preventing the formation of sinuous channels amongst the muscles of the limb, and renders the subsequent operation more easy, and perhaps enables the surgeon to perform it sooner. If the pressure of the lips of the wound closes the mouth of the tube, and thus prevents the escape of the discharge, it is the practice at this Hospital to slit up the part external to the wound, and tie down the two ends in different directions, whereby the mouth of the tube is kept permanently dilated. We are informed that the foreign body in the wound does not appear to set up any irritation.

Case I. John Mercer, aged 43, was admitted on May 18th, under Mr. Bowman's care, on account of an abscess on the right side of the chest. It was situated over the lower ribs, and could not be distinctly proved to communicate with the chest. It is not necessary for our present purpose to go minutely into the history or symptoms of the case with reference to this. It is sufficient to say that, the nature of the swelling being very evident, it was punctured on May 23rd; and a probe was at first passed into its cavity every day. As, however, the matter bagged, it became necessary to enlarge the puncture by an incision about three-quarters of an inch in length, and the abscess was treated in the usual manner; but, as the pus still continued to collect, a counter-opening was made on July 11th, and a drainage-tube passed from one opening to the other. This afforded a ready exit for the discharge, which improved from a thin serous fluid to laudable pus; and he continued comfortable in respect of this abscess. Meanwhile, a few days after his admission, he was found to be labouring under pain over the upper third of the right fibula; and, on examination, considerable swelling was found in this part, consolidating all the tissues of the leg, but without distinct fluctuation. This, however, soon made its appearance (without any symptom besides pain in the part); and then (June 1st) a bag of fluid was formed in the situation of the peronæi muscles, which could be rolled upon the bone beneath them. A longitudinal incision, about three-fourths of an inch long, gave exit to some pus mixed with blood, which seemed to be infiltrated in the substance of the muscles. On June 20th, as matter was presenting below the orifice, an opening was made at this spot, four inches below the original incision, and a drainage-tube of small size was passed from one to the other. This, however, was not strong enough to resist the pressure of the parts, and consequently was liable to get closed. A thicker and stronger one was accordingly made use of. This latter, however, was also in its turn obliterated by the pressure of the parts, and matter again bagged below the lower opening. The tube was accordingly passed through at this lower level, being now split up outside the wound, and its halves bound down, so as to keep the cavity of the tube permanently open by traction on each of its sides. This was done on July 4th, and seemed quite to succeed; so that in a few days (July 13th) it was found feasible to withdraw the tube from the lower opening, as it was granulating and seemed inclined to heal, and pass it into the second. Pressure was at the same time made below this by strips of amadou plaister and a bandage, to obliterate the lower part of the cavity. When last seen (Aug. 4th), the tube had been withdrawn, the parts appeared quite consolidated, and the wounds were nearly healed. He was in much better health, and the abscess in the side seemed to be contracting.

Case II. Joseph Patrick, aged 36, was originally admitted under the physician for pain in the back, supposed to be of a rheumatic nature; and was sent down to the surgeon's ward, when it was found that this pain depended on caries of the vertebræ, and was accompanied by psoas abscess. The abscess was then of about the size of an orange, situated at the inner part of the right thigh. It was opened on June 13th, by an incision about half an inch in length, from which about four ounces of a creamy-looking pus escaped. A drainage-tube, one-sixth of an inch in diameter, was introduced, and a poultice applied. This tube was kept in without any bad symptom, being removed occasionally to be cleaned; and all connected with the abscess went on well. A probe passed upwards eight inches from the wound to a point about an inch above Poupart's ligament. It was necessary to put on pressure in the femoral triangle, as the matter had formed a pouch there, into which, as he lay with the knee raised, it used to regurgitate. The abscess has continued steadily to contract, and the probe does not now (Aug. 5th) pass above the level of Poupart's ligament. He can also now raise the limb, which he could not do before. There is now some pain and hardness in the opposite thigh.

Case III. Elizabeth Hart, aged 21, was admitted under Mr. Bowman's care on June 12th, on account of pain and effusion into the knee-joint, depending apparently on an acute attack of synovitis. By active treatment, however, the effusion in the joint was reduced; and then (June 20th) swelling showed itself on the outer side of the femur, external to the knee-joint, in which deep fluctuation could be felt. This was opened by an incision an inch and a half in depth, and led into a large suppurating cavity, reaching half-way to the trochanter. She complained of acute pain for some days after the opening, which, however, gradually decreased as the discharge continued. An elastic tube was introduced a few days afterwards, and the part soon became quite easy, so that she could flex the knee without pain. On July 11th, Mr. Bowman again ex-

amined the part, and found the cause of the mischief in a piece of necrosed bone at the back of the popliteal space of the femur, which was quite firm. A drainage-tube of small size was passed down to this, in order to obviate any more bagging of the pus; and the discharge soon became very slight in quantity. On July 27th, she was discharged, relieved for the present, to come again at a future opportunity, in order to have the dead bone removed.

UNIVERSITY COLLEGE HOSPITAL.

I. HYDRARTHROSIS OF THE KNEE: BONY PLATE FORMED IN THE SYNOVIAL MEMBRANE.

Under the care of J. Erichsen, Esq.

Wm. W., aged 50, was admitted, under Mr. Erichsen's care, on December 2nd, 1858, on account of disease of the right knee-joint. It seemed that, two years previously, his knee swelled, and in about a month's time increased to a very large size. The pain then became intense at intervals; and since that time the size of his knee has varied considerably, and there has occasionally been œdema of the leg. On admission, the joint could not be perfectly straightened. The knee measured across the patella three inches more than on the sound side, forming a fluctuating swelling, of an ovoid shape, about the size of a child's head, pretty even in outline, but projecting somewhat on its inner and under part, corresponding with the head of the tibia, and bound down by the internal lateral ligament. The patella could be felt floating at the upper surface, and rather to the outside of the tumour. On depressing the lower border of the patella, and pushing it upwards, crepitus could be felt, and smart pain was occasioned. Half an inch above and outside of the patella, a firm body could be recognised, which was freely moveable, although attached. It felt larger and thicker than the patella. This body was marked by a large and deep sulcus in the axis of the limb, across its centre. There was fluid over the lower border of this body, and in the sulcus. The skin was equally moveable over the foreign body as over the patella. There was no fluid immediately beneath it. The fluid caused the tumour to bulge at the parts where it was relieved from pressure.

The knee was painted with the caustic tincture of iodine, and his limb firmly bandaged over cotton wool as high as the knee.

He remained in the Hospital till January 20th, and was then directed to attend as an out-patient. The knee was softer and a little smaller than when he came in. His general health was at that time pretty good. He was, however, re-admitted on January 24th, with some anomalous symptoms; and was shortly afterwards transferred to the physician's charge, on account of epileptic fits, of which he soon afterwards died.

On *post mortem* examination, the foreign body was found to be a plate of bone, of the shape of the patella, quite thin in every part, apparently moulded to the outer surface of the condyle. The capsule was distended with fluid, which exhibited exudation-cells under the microscope. The articular surface of the femur was in great part covered with blood-red granulations, giving it a villous aspect. In some of the granulations, small deposits of bone were found. The bony plate exhibited the characters of true bone under the microscope. A separate and smaller one, of the same nature, was found below it.

II. HYDROCELE OF THE TUNICA VAGINALIS, RESISTING THE USUAL REMEDIES: CURE BY SETON.

Under the care of J. Erichsen, Esq.

Christopher J., aged 20, was admitted, under Mr. Erichsen's care, on February 16th, 1859. About ten years before, he received a blow upon the right testicle, which caused him to faint. He was very weak for some time afterwards, and "found that the testicle had increased to twice its natural size". About two years ago, in jumping on horseback, he again injured the same testicle. This was followed by similar symptoms. He felt weak, and there was a swelling in his scrotum, which "was at first soft, but gradually became hard". Two months before admission, he strained himself in lifting a weight; since which time he experienced a sensation of great heaviness in the testicle, and increased weakness. He had also, on admission, a discharge from the urethra; but he denied that it was gonorrhœal. The swelling in the scrotum proceeded from hydrocele.

The discharge was treated with a weak injection of chloride of zinc; and bicarbonate of potash with henbane was given.

On February 28th, a silver wire was introduced through the cavity of the tunica vaginalis, the fluid having been drawn off. The first wire was allowed to stay in a week, and then a second was put in, the first still remaining. Both were withdrawn on March 9th. They caused a good deal of inflammation in the scrotum; but on March 25th the hydrocele had refilled, and it was accordingly tapped and injected with tincture of iodine. The fluid, however, again formed; and there was no appearance of a radical cure. Accordingly, on April 13th, a silk seton was introduced. This caused great pain during the night, which was not relieved by laudanum; and the testicle became greatly inflamed. The seton was removed thirty hours after its introduction, and about eight ounces of blood were drawn from the veins of the scrotum, with much relief. The patient was also put upon tartar emetic and low diet. He went on well after this; and on May 6th it is stated that the testicle was much diminished in size, though still hard and rather painful. There was a little purulent discharge from the track of the seton-threads.

The man was soon afterwards discharged, and was seen about the latter end of June, quite well.

III. COMPOUND DISLOCATION OF THE WRIST: DISLOCATION OF THE SHOULDER.

Under the care of J. Erichsen, Esq.

William H., aged 50, was admitted on April 23rd, under Mr. Erichsen's care. He had had his right arm drawn into the machinery of a steam-engine in motion, and was brought to the Hospital shortly afterwards. On his arrival there, at 6 P.M., it was found that there was a compound dislocation of the wrist, the carpus being thrown backwards; the flexor tendons of the wrist had been ruptured, and the soft parts torn away from the front of the forearm to the extent of three or four inches, leaving a large cavity amongst the muscles, which were much lacerated, in which three or four fingers could be placed. Neither the radial nor ulnar arteries could be seen pulsating; so that it was apprehended that they were lacerated. The right shoulder-joint had been dislocated, and the head of the humerus thrown into the axilla. There was a good deal of ecchymosis along the anterior fold of the axilla, and no pulsation could be felt in the brachial artery. The limb was cold and numb. Still the patient did not appear to be suffering, and made no complaint. Slight attempts only at reduction were made on his admission by the house surgeon; and then the limb was steadied with a splint, the patient put to bed, and Mr. Erichsen sent for. On Mr. Erichsen's arrival, he found that the patient had lost no blood from the wound, but that the ecchymosis had greatly increased. On consultation with his colleagues, Mr. Erichsen determined to administer chloroform, reduce the dislocation, and then be prepared to amputate at the shoulder-joint, should pulsation not return in the brachial artery on the reduction of the head of the bone; but if it did, then merely to remove the hand. The dislocation of the humerus was reduced accordingly, and this had the effect of immediately restoring the pulsation of the brachial artery. No bleeding, however, occurred from the wound. The forearm was then amputated by the flap operation. The operation was followed by slight symptoms of delirium tremens, and he had a slight attack of erysipelas. This yielded, however, to appropriate remedies. The shoulder was the seat of very great ecchymosis.

Nothing remarkable occurred in the further progress of this case. One of the ligatures remained on for a long while. On July 5th, it is noted that the patient is quite well, and the wound united.

Remarks. The curious feature in this case was, that the pressure caused by the dislocated head of the bone on the axillary artery—a complication probably existing frequently in dislocation of the shoulder, but not found, because not looked for—threw at first some doubt on the nature of the accident. The injury was of such a nature as might easily have resulted in a rupture of the axillary vessels; in fact, a case of rupture of the popliteal vessels from a similar cause, under Mr. Cæsar Hawkins's care, was recently reported in this Journal. Fortunately, the present injury was not of so grave a nature; and beyond the loss of the hand, which the injury of the soft parts rendered inevitable, he recovered without further mutilation.

WEST DERBY UNION HOSPITAL.

CASE OF OPIUM POISONING.

By Edward Lister, Esq., Surgeon to the Hospital.

Mary Ann G., aged 30, was admitted into the above hospital, on April 5th, 1859, suffering from the effects of an over-dose of laudanum. For some time before, she had been in an unsettled state of mind, and about half-past four o'clock on the above date, she sent a child to a neighbouring druggist for two shillings worth of laudanum; the whole of which she took about five o'clock. She then went up stairs, and was found some time afterwards lying on the floor in a state of unconsciousness. A surgeon residing in the neighbourhood was sent for at about half past seven or eight o'clock; and the stomach-pump and emetics were used, no doubt, with some benefit. On my seeing her, however, in the hospital about nine o'clock, she was in a state of total insensibility; the pupils were firmly contracted, and the retina was quite insensible to the stimulus of a lighted candle; the breathing was laboured and stertorous; the pulse slow and feeble; and all efforts to rouse her appeared useless. The stomach-pump, emetics, strong liquor ammoniæ and other stimulants, were used; the shoulders, back, palms of the hands, and soles of the feet, were flogged with wet towels; four powerful magnetic batteries were also brought into use, and applied to the spine and other sensitive parts of the body. The insensibility continued until about one o'clock A.M., when she gradually began to show signs of returning consciousness, and answered questions in monosyllables. Vomiting was occasionally excited, which appeared to be of much benefit. She was then kept constantly moving about; but if allowed to rest, she would immediately return into a lethargic condition.

At three o'clock, the sensibility had so far returned, that I considered she could be left with safety, giving strict injunctions to the nurses to keep her awake until I saw her again. She had a shower-bath given about nine o'clock, which had the effect of refreshing her considerably.

On visiting her again at ten o'clock, she was much better; and on the following day was quite well. A constipated condition of the bowels with headache were the only remaining symptoms.

The druggist who supplied the laudanum stated that there were two fluidounces. The husband saw her swallow it; but, being in a state of intoxication, he did not succeed in getting any part of it from her, nor did he give any alarm, and she was accidentally found by a woman coming into the house two hours afterwards.

The principal point in this case appears to be the length of time such a large quantity of laudanum remained on the stomach, and recovery afterwards taking place; and I am of opinion that her life was saved by the continued use of the galvanic batteries.

Original Communications.

REMARKABLE CASE OF VARICELLA IN AN ADULT.

By W. F. Cleveland, Esq.

Mrs. T., aged 35, of fair complexion, highly excitable, had been suffering for about a week from slight gastric derangement, when she was seized in the middle of the day, May 21st, 1859, with shivering, which did not entirely leave her until she went to bed at night.

Next day she presented the usual symptoms of a smart febrile attack, with severe headache and drowsiness. As the bowels were rather confined, an alterative pill and black draught were prescribed; and, after this had acted, she was to take a saline diaphoretic mixture every four or six hours. There was now redness and slight fulness of the face.

May 23rd. She complained of great itching about the chest and abdomen, for which a lotion, composed of sulphate of zinc dissolved in camphor mixture, was applied with benefit. There were now observed numerous small red protuberances on the skin. As the bowels had not been freely relieved, she was ordered to repeat the black draught.

The same evening several vesicles, filled with white transparent lymph, appeared on the chest, abdomen, and face; and the opinion that the case was one of varicella was now given. The eruption was out freely on the scalp.

May 24th. The eruption was much more copious, and consisted mostly of pustules. The eruption was now also on the tongue, gums, and lining membrane of the cheeks; also on the meatus auditorius externus of both ears. She complained of great pain in the ears and on each side of the neck. The eruptive fever was less.

May 25th. The face was so swollen and disfigured, and there were so many pustules, that some doubt whether the case was not one of variola was felt. My friend, Dr. J. Ridge, was therefore requested to meet me in consultation; and after careful examination, he confirmed my view that the patient had an unusual attack of varicella. A saline draught was continued three times a day. The eruption was now out on the arms and legs; but only slightly in comparison with the face and body. The pustules outnumbered the vesicles.

May 26th. Some of the pustules were beginning to shrink and pucker. Several on the face, chest, and abdomen, were confluent. I now counted the vesicles and pustules on the face, taking half of it at a time. On one side there were a hundred and twenty of these eruptive spots, and on the other side eighty.

May 27th. There were some fresh vesicles on the arms and legs; but the eruption generally was beginning to scab. She complained of feeling very weak. She was ordered to take wine and beef-tea freely, and a fourth part of a mixture, containing sesquicarbonate of ammonia and chloric ether, three times a day.

June 21st. There are six or seven rather deep pits on the face where the pustules were confluent. In all other respects she is quite recovered.

She had been vaccinated in infancy, and the ordinary cicatrices were apparent.

Remarks. Chicken-pox is by no means common in the adult. Dr. Watson writes, "The disorder is almost peculiar to infants and children of tender years. Willan has, however, described one unambiguous example in a gentleman thirty years old; and another genuine instance was seen by Dr. Gregory at the Small-pox Hospital, in the person of an adult female."

Some may entertain a doubt, as I did myself on the third day, whether the case was not one of modified small-pox (varioloid?); but when we regard the rapidity with which the pimples were converted into vesicles; the presence throughout the eruption of vesicles containing clear transparent lymph, which is said never to be the case in varioloid; the early period at which the pustules began to pucker and scab; the absence of the depressed centre so characteristic of the variolous vesicle; together with the entire absence of pain in the loins, and the disproportionate amount of constitutional disturbance prior to the development of such a copious eruption, as well as the subsequent progress and termination of the case, the evidence in favour of varicella seems to preponderate.

It is generally laid down by authors that chicken-pox differs so greatly from small-pox in not attacking the face. Dr. Heberden is said never to have been able to count more than twelve eruptive spots; but in the case I have detailed there were at the least two hundred. It may be mentioned that at the time this case occurred small-pox prevailed in the neighbourhood; but I had not attended any cases myself, nor had this lady been in communication with anyone suffering from the disease. It may also be worthy of note that there had been an offensive smell from a drain for some weeks past in the room chiefly inhabited by my patient, and as the temperature of the weather had increased, she was obliged to go into another house while the necessary repairs were progressing. It was just after removing into lodgings that she was taken ill.

Now, for those who hold the opinion that varicella is a perfectly distinct disease from variola, and who look upon this case as being a genuine, albeit a severe, instance of a mixed variety of the former malady; it may be a question of interest whether a specific miasm may have been included in the offensive emanation from the drain.

23, Beaufoy Terrace, W.

ON SOME OF THE PRINCIPAL EVENTS IN THE CLINICAL HISTORY OF ASTHMA.

By HYDE SALTER, M.D., F.R.S., Fellow of the Royal College of Physicians, and Assistant-Physician to Charing Cross Hospital.

VIII.—The Asthmatic Physique; its Distinctiveness; its Characteristic Gait, Physiognomy, and Configuration; Rationale of the Asthmatic Spinal Curvature; Peculiar Chest of young Asthmatics.

Asthma is a disease that stamps on the body its own indelible marks. So characteristic and unmistakeable is the physique of asthma, so plainly, so legibly does the asthmatic bear about with him the impress of his disease, that any one who has once observed it will never fail to recognise it, and would be safe in basing a diagnosis on its unaided testimony. In confirmation of this, I may mention a circumstance or two that have occurred to myself.

I frequently meet in the streets of this city a distinguished savant, who is a great sufferer from asthma, and if I were to meet him at Pekin I should know him to be an asthmatic. The first time I saw him, I was walking behind him, but immediately recognised the characteristic configuration. So certain did I feel of it, and so curious was I to see if I could derive from his face any confirmation of my impression, that I overtook him, and as I passed, saw at once who he was, from his resemblance to a photograph I had seen of him; and then I remembered what I had heard about his malady. This previous knowledge, however, could have had nothing to do with my impression, as at first I had no idea who he was, in fact had never seen him before.

On another occasion, I was going down to Brixton in an omnibus, and sitting next to me was a gentleman, whose figure and appearance very much struck me. He looked well in the face, was breathing without the slightest difficulty, and was engaged in animated conversation with his next neighbour. But I felt sure he was an asthmatic; he was exceedingly thin, his shoulders very high, and his back so rounded, that though quite a tall man, he sat lower, a good deal, than I did. He left the omnibus before me, and in walking his stooping gait and rounded back were still more conspicuous. When I left the omnibus, I asked the conductor "If he knew that tall, thin, stooping gentleman" that got out at such and such a place. "Oh yes, he said," it is Mr.— of —. "Do you know," I asked, "if he is subject to a complaint in his chest, that gives him fits of difficult breathing?" "Oh! yes," he replied, "and has been for a long time—for years; sometimes he can hardly walk to the omnibus."

Here then are two cases in which I recognised the presence of asthma, merely by the changes in the physiognomy and configuration which it produces. I have done the same thing in several other instances. I often say to myself as I pass an individual in the street with the characteristic configuration—"You are an asthmatic."

What then is this unmistakeable impress of asthma? How is it to be described? In what peculiarities does it consist? Partly those of the figure, partly those of the face. In configuration, the asthmatic is round-backed, high-shouldered, and stooping; but while the body is bent forwards the head is thrown back, and buried, as it were, between the elevated shoulders. There is no movement or pliancy in the body, but the chest is fixed and rigid, like a box, and from it the arms depend, hanging motionless, or swinging like two pump-handles, rather thrown back, and bent at the elbows. In walking, the legs seem the only part of the body that moves. From the roundness of the back, the asthmatic, for his height, always sits low. Those who have suffered from asthma long, are almost invariably thin, often to a degree amounting to emaciation; their limbs are attenuated and bony, every rib can be counted, and their clothes hang loosely on them. From the entire absence of subcutaneous fat, the superficial veins are very conspicuous; though it is possible that venous obstruction, from impeded circulation through the lungs, may have something to do with their prominence,—that it may be, in fact, slight general varix. The prominence and tortuousness of the veins is sometimes remarkable, especially in old asthmatics. The hands of an asthmatic are very characteristic; they are cold, blue, thin, and veiny. This blueness and coldness of the extremities is partly due, no doubt, to the feebleness of the circulation in them, but in part, especially if there is some permanent dyspnœa, or the patient is at the time suffering from an attack, to imperfect decarbonisation of the blood.

But this blueness of surface is not confined to the extremities; the complexion of the asthmatic is often distinctly cyanotic—a slight but perceptible duskiness. Where, however, there is no dyspnœa between the attacks, and the intervals are long, especially in young subjects, I do not think the complexion is perceptibly affected.

The face has generally rather an anxious expression, and often bears an aspect of age greater than the real age of the patient; even if not dusky, it is always pale, and like the rest of the body, is thin—the cheeks are hollow, and the lines of the face are deeply marked. The mouth is generally open, and the jaw rather hanging. There is a peculiarity about the eyes to which I would call particular attention, to which I often *have* directed attention among the patients at the hospital, and on the strength of which alone I have frequently diagnosed asthma; they are turgid, watery, and prominent. I believe this to depend, like the enlargement of the superficial veins, upon venous obstruction; the turgidity is that of the veins of the conjunctiva; the wateriness is due to an unusual amount of mucus and tears, which the engorged mucous membrane and lacrymal gland pour out, while the prominence is due to the turgidity of the veins of the orbit. Wherever I find this appearance of the eyes in connexion with chronic asthma, I look out for dilated right heart.

The asthmatic's voice is often very peculiar; it is feeble, and slightly hoarse and rough; he speaks as a healthy person would speak, if he were to expire as long as he could and then begin to speak,—as if he were making use of the last breath in his lungs; and it is indeed to the small respiratory resource that he has for speech that this feebleness and roughness of the voice is due. His sentences are short, and frequently interrupted by a single dry cough.

Many of the external characteristics of asthma that I have enumerated, such as the gait, the dusky complexion, the eyes, the voice, will be seen at once to be those rather of chronic dyspnœa than of asthma, and I would so far qualify their value as diagnostic signs. Indeed, their value is rather generic than specific. In the purest cases of spasmodic asthma, in which there is no trace of dyspnœa in the intervals, they are the least marked, and are the most conspicuous in those which are complicated with some chronic bronchitis or bronchial congestion, so that I look upon their absence as a good sign, as a sign that the lungs are sound and unscathed; and it is because asthma seldom exists long without producing such changes as permanently embarrass the breathing, that these changes in the physiognomy and configuration come to be signs of asthma; they are just as much a part of its clinical history as those internal changes in the lungs and heart, of which they are the sure sign and accompaniment. I have, however, seen the asthmatic physique very strongly marked in some cases where the lungs were organically sound; but these were always cases where the paroxsyms were very long, lasting for days or weeks, so that the efficient cause was in continued operation for a long time; or else they were cases in which the asthma had been very severe in childhood, when the figure was forming, so that it got *set* in the asthmatic shape, and, although the disease afterwards quite disappeared, the figure never recovered itself. Such cases carry a certain highness of shoulders and roundness of back with them to the end of their days.

The degree of deformity of the back is, in some cases, truly remarkable; and (although I have not examined any of the spines of these cases after death) such as I cannot but think must involve some anatomical change in the bones or intervertebral discs. The curvature is always direct antero-posterior, never lateral, and involves the middle or lower dorsal vertebræ. In one case, I have seen the angle made by the curvature almost as considerable as an angle of a hundred degrees. Now, it is certain that such a change in direction as this, almost a right angle, effected within the length of a few vertebræ, could only be produced by a great shortening of the anterior aspect of the column, involving a vertical shortening of the anterior part of its separate elements—vertebræ, or intervertebral discs, or both—converting the bodies of the

vertebræ, or the discs of fibro-cartilage between them, into wedges, as shown in the accompanying diagram.

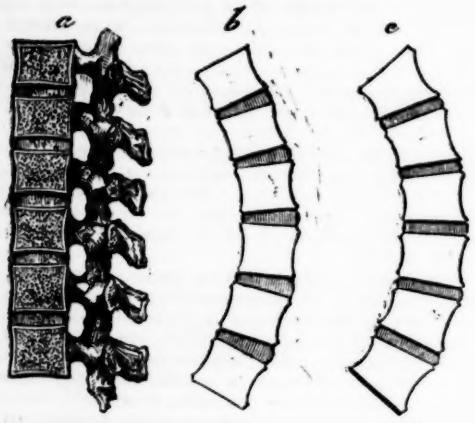

Fig. 1.
a. Vertical Section of the Spinal Column.
b and *c*. Anterior Spinal Curvature produced by absorption from pressure in the anterior parts, in the one, of the intervertebral discs, in the other, of the bodies of the vertebræ.

Now, the agency by which a disease affecting the air-passages produces this curvature of the spinal column was not very clear to me, and it was long before I arrived at what I believe to be its true explanation. It certainly was not a change of form having for its object the enlargement of the thoracic cavity, like the barrel-chest of emphysema; it was manifest that such a deformity must materially prejudice the size, the shape, and the movements of the thorax. It was equally certain that there was no spinal disease; that the curvature was the result of repeated attacks of asthma; that it was the permanent retention and constant aggravation of the asthmatic stoop which always exists during the paroxysms. But, beyond this, its nature and causation did not appear intelligible. At length I was guided to what appears to me to be its true *rationale* by the following considerations.

1. Pressure produces absorption of the part pressed on; *e.g.*, the absorption of the sternum and ribs in aneurism.

2. In stooping—*i. e.*, in forward curvature—there is vertical pressure along the anterior face of the spinal column; *i.e.*, its elements exercise vertical pressure upon one another in their anterior portions.

3. The unsupported spine tends to fall forward—*i.e.*, to anterior curvature; the whole array of the muscles of the back are erectors of it.

4. When the muscles of extraordinary respiration are engaged in extraordinary respiratory efforts, they cease to play their ordinary *rôle*: *e.g.*, the omo-hyoid muscle is ordinarily a depressor of the hyoid bone, but in asthma it is an elevator of the scapula, and powerless to depress the hyoid bone: the sterno-mastoid is ordinarily a rotator and anterior flexor of the head; in asthma, it is an elevator of the sternum, and powerless to move the head.

I infer, therefore, that the muscles of the back, being engrossed during the asthmatic paroxysm with their violent respiratory labour, cease to act as erectors of the spine. The back is, therefore, unsupported, and obeys its natural tendency to fall forward. This, I am sure, is the true explanation of the asthmatic stoop; and we see, consistently with this view, that any effort to straighten the back is immediately attended with additional distress, because the muscles are temporarily withdrawn from their respiratory exertions; and that the stooping is always in proportion to the dyspnœa—that is, in proportion to the degree in which the muscles of the back are engrossed by the breathing. The back being, therefore, left thus to itself, and falling thus forward, the anterior parts of the bodies of the vertebræ and intervertebral discs are pressed together; and if the asthmatic state is continued long, and occurs frequently, this pressure is so protracted that it produces absorption of this part of the bodies of the vertebræ, or the intervertebral fibro-cartilages, or both; they become wedge-shaped, and the temporary stoop becomes a permanent curvature.

Now, I admit that I have never examined, *post mortem*, the spine in cases of this asthmatic deformity; and I merely offer this hypothesis as a reasonable explanation. In cases in which I have seen it, there has not existed the slightest ground for believing that there was any spinal disease. In the case of William Burr, before the asthma came on, the back was straight, the patient tall; but as the asthma became worse and the attacks longer and more frequent, the stoop, which he always had during the attacks, went off less and less in the intervals, and gradually became more and more considerable, till it settled into the permanent deformity which now exists, and which, though his legs are long, has made him quite a short man. No pain is experienced in the back in these cases (except an aching of the dorsal muscles during the asthma, from their violent respiratory exertion). Moreover, the curvature is never confined to a single vertebra, or to two or three, and is never angular; it is rounded and gradual, and involves a considerable portion of the back. In such an absence of all evidence of spinal disease, and with an amount of curvature in some cases making some change of form in the elements of the spine a physical necessity, I do not see what explanation can be offered other than that I have given. If correct, it is certainly interesting; it has the interest of all facts which elucidate a *modus operandi*, and furnish the connecting links between associated, but apparently diverse phenomena.

When my mind was engaged in thinking on this subject, a case of another nature occurred to me which confirmed my ideas, and seemed to show that mere loss of power on the part of the muscles of the back was adequate to the production of anterior spinal curvature. A man in the prime of life, with an erect figure, who had never had anything the matter with his spine in any way, was seized with severe rheumatism in the muscles of the back, and the pain was so aggravated and rendered so intense by attempting to stand upright, that the muscles refused to perform their office; the unsupported spine bent forward as far as its elasticity would permit, and the back became round and the gait stooping, like that of an old man. This went on for some months, without any decided curvature appearing; at length, distinct and gradually increasing curvature manifested itself, involving the seventh and eighth dorsal vertebræ, and to such an extent as to show that their bodies must have undergone some change of shape. The patient recovered from his rheumatism, regained the power of supporting his back, and the curvature was arrested; but it has never disappeared.

If, then, loss of power of the spinal muscles, paralysed by rheumatic pain, is adequate to the production of organic curvature in the way that I have supposed, why should not their paralysis, as erectors of the spine, from their being engrossed in their respiratory action, be attended with the same result?

In young asthmatics who have suffered from their disease in infancy, I have observed a configuration of chest which I have seen, to a certain extent, in cachectic rickety children who have not suffered from asthma, but which I am not sure I have ever seen in children who have not suffered from any chest-affection. I am not sure, therefore, what share in the production of the deformity ought to be assigned to the asthma, or what to the deficient strength of the bony parietes of the chest; or whether imperfect ossification and chest-mischief are both necessary for its production. What inclines me to think that the latter cause is the chief one (or, at least, an essential one), is, that the deformity is just such as would be produced where there was an impediment to inspiration, and where violent but ineffectual inspiratory efforts were made by weak parietes—just such, in fact, as one sees in the inspiration of atelectasis or laryngitis.

In these cases, the upper part of the chest—above the fourth or fifth rib—is naturally shaped; but below that level, on each side of the sternum and along the margin of the false ribs, corresponding to the cartilages of the seventh, eighth, ninth, and tenth, the surface, instead of being convex and full, is excavated in two hollows. These hollows are bounded internally by the lower part of the sternum, which stands forward between them in a ridge, and externally, by the extremities of the ossified portions of the rib, which form a ridge on the outside. (Fig. 2.) The situation of this flattening in the thoracic parietes—its coincidence with the cartilages of the ribs—suggests the way in which it is produced, and explains how asthma might cause it. I take the method of its production to be this:—When there is any impediment to the ingress of air into the lungs—from narrowed bronchial tubes, as in asthma, or any other way—the air cannot rush in so fast as the thoracic parietes are expanded, a partial vacuum is therefore formed in the chest, and the pressure of the air within it no longer balances that of the air without. This unbalanced external atmospheric pressure

presses on the whole of the thoracic parietes equally, and drives in the most yielding; in other words, the bony ribs, raised against it by the respiratory muscles, are able to resist it, but the soft cartilaginous portions are not. It must be remembered, too, that the diaphragm is attached along the margins of all the

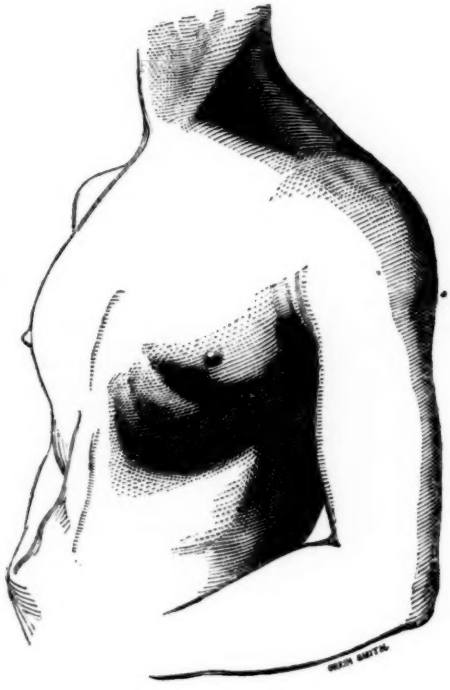

Fig. 2.
Pigeon-chest in Asthma dating from infancy.

ribs below the fifth; its strong contractions during the asthmatic paroxysms would tend therefore to draw them in, and being less opposed by their yielding cartilaginous portions, would concur in the production of this change of shape. This condition would, of course, at first only last as long as the paroxysm; but if the attacks were frequent and severe at a time when the cartilaginous portion of the ribs was considerable and the figure forming, one can easily understand how the deformity would become permanent, and how asthma would in this way produce such a change in the form of the chest as I have been describing. Still more easily could we explain its production by asthma, if we suppose asthma capable of giving rise to pulmonary collapse, in the way I have suggested.

But the actual event to which I think this form of chest directly points—the particular morbid change that it immediately implies—is pulmonary collapse. One cannot but be struck, on looking at such chests, with their exact resemblance (except in the absence of what one may call the diaphragmatic stricture—the drawing in of the attachment of the diaphragm at each inspiration, as if a ligature were put round the body) to the shape of the chest in the inspiration of infants suffering from bronchial plugging, or its established result, atelectasis, or croup, or laryngitis, or any affection that, preventing the ingress of air and the expansion of the lung, refuses to allow the parietes to follow the movements of the inspiratory muscles.* Pulmonary collapse would certainly be a more potent cause of this deformity than unaided asthma, because it is a more permanent condition and involves a more complete inexpansibility of lung, and therefore would more effectually resist the inspiration-movement of the softer parts of the parietes.

In as far, then, as this heteromorphism points to atelectasis, its occurrence in cases of asthma tends to confirm what I have already said of the tendency and sufficiency of that disease to produce this condition of lung. But remembering that it is only in cases that have become asthmatic young that this state of chest is well marked, and remembering how commonly asthma dates in the young from catarrhal bronchitis, from the bronchitis of measles, or from what has been called "whooping cough with bronchitis", which is nothing more than whooping cough in which the bronchitic portion of the malady has extended far and severely into the bronchial tree—remembering this, I say, one cannot but see it possible that, at least in many cases, the pulmonary collapse may have been produced in the ordinary way, by mucous plugging of the bronchial tubes from the primary bronchitis, and not from the consequential asthma, in the way I have suggested. I have seen this deformity strongly marked in cases where ordinary bronchitis had never occurred, and where the patient was entirely free from bronchitic complication or tendency; but then the asthma dated from whooping cough in infancy—and, in my opinion, no form of bronchitis has so great tendency to generate collapse as that of whooping cough.

Dr. Gairdner, of Edinburgh, in his excellent papers on the "Pathological Anatomy of Bronchitis", alludes to this deformity of chest as an occasional permanent result of pulmonary collapse. "In rickety individuals," he says, "it is not only more marked, but apt to become permanent, especially when such subjects are affected with any considerable or persistent bronchitic affection. In such cases, the reversed movement of the ribs is stereotyped, as it were, in the form of chest, called *pigeon-chest*, in which the sternum is protruded, particularly below, and the whole lateral region, including also the lower costal cartilages in front, flattened, or even at some points rendered irregularly concave."

One result of this collapse of the cartilages of the ribs is that they do not stand out from the sternum as they should; but decline from it at too acute an angle, narrowing and elongating the scrobiculus cordis.

Fig 2, a very good example of this configuration, is taken from a photograph of an asthmatic who suffered from his disease from the age of three months. The condition has existed from his earliest recollection, and during his childhood was much more considerable, for since he has grown up his disease has left him, and the shape of his chest has much improved.

* I have seen it strongly marked in the inspiration of laryngitis, even in adults.

ILLUSTRATIONS OF THE USE OF THE OPHTHALMOSCOPE.

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[*Continued from page 624.*]

Retina.

The affections called *amaurosis* and *amblyopia* will be noted together. Although these are not accurate scientific terms, they do well enough, in the present state of our knowledge, to indicate the physical symptoms and the derangement of vision attendant upon a well known condition, involving local and constitutional changes, the causes of which are, in the very large majority of cases, not ascertainable. It is only now that, since the introduction of examination by the ophthalmoscope, we are beginning to discover the local conditions upon which the derangements depend. In time, we shall be able to know something of the efficient and proximate causes of these conditions. We may even at present classify, in most instances, amaurosis as being organic or incurable, or only functional or curable; as being caused by disease of the brain or of the optic nerve, retina, or choroid, principally; and this we do chiefly by our observation of the state of the fundus, and of the condition of the optic papilla and its vicinity. If the disease, however, lie somewhere between the optic nerve and the brain, it will not be discoverable by the ophthalmoscope; and if we find changes in the optic nerve, we may not know whether this is the essential disease, or whether it may be only symptomatic of diseased brain. It may be observed, that where we find the papilla compressed and atrophied, we have one indication for concluding that the efficient cause of the disease is external to the globe. The great practical utility of this improved means of diagnosis is, that whereas formerly, in a very large number of cases of amaurosis, various heroic measures were resorted to, in the way both of depletion and stimulation, where we find a state of organic amaurosis, or have seen strong reason to suspect that such a state exists, we shall avoid any such heroic remedies. On the other hand, whereas in many cases of amaurosis, they were set down from the first as incurable, and

treatment was considered absolutely useless, we shall now find a certain proportion in which we have every reason to conclude that the cause is of a functional or curable nature, and that appropriate treatment, sometimes of an active character, in the way of depletion or stimulation, will be likely to be attended with the best results.

Amaurosis in connexion with Albuminuria. We shall see portions of the retina, generally near the papilla, covered with small patches of a vivid red colour; and in these, again, small white spots, as in retinal apoplexies. These spots enlarge as the red patch diminishes, and seem to be due to absorption of pigment at the same time with that of effused blood; and concurrently we may have signs of œdema with infiltration of the papilla, white tracts in the course of the retinal vessels; and this may end in fatty transformation of the retina. Vision will become deteriorated according as the macula lutea becomes encroached upon, and will be gradually lost altogether. Desmarres figures a case in vol. iii, p. 520. Hogg, at p. 70, notices some observations of Quadri, who found, in some cases, the retina almost entirely detached, with œdema; in other cases, great hyperæmia, sanguineous effusion, fatty deposits, etc.

Amaurosis with Glycosuria or Diabetes Mellitus. Observers have found the fundus of a pale grey colour; turbid vitreous humour; patches, the result of old ecchymoses which have become absorbed; patches of accumulated pigment; maceration of pigment. Cases are figured at pages 524 and 526, Desmarres, vol. iii.

Amaurosis with Syphilis. Two things are generally observable in eye diseases, where the constitution has become affected by a syphilitic taint. First, there are flocculi floating in the vitreous humour; secondly, a disorganised state of the retinal circulation. One or the other, or both these, in combination with other morbid changes, were observed in the following cases:—In a case of Mr. Poland's, May 31st, 1858 (see Bader, in *Ophthalmic Hospital Reports*, October 1858), there was primary syphilis. The right eye was affected with frequent inflammation; the left suddenly failed lately; there was no pain or inflammation. The fundus in both was lighted up naturally. The retina was hazy; there were no flocculi in the vitreous body. The papilla was ill defined, surrounded by a yellow halo; its surface was grey, with a greyish white centre. The vessels were irregularly dilated; at certain parts of the fundus, in the left eye, chiefly at and near the macula lutea, were brilliant yellow white and brown patches. Dr. Bader remarks, that the brilliant cherry red œdematous appearance of the fundus, with well defined optic papilla, are often met with in syphilitic persons, particularly if they have taken much mercury or quinine. Vision in this case was very bad. In a case of Mr. Wordsworth's, January 27th, 1857 (see Plates 4 and 5, *Ophthalmic Hospital Reports*, October 1858)—inflammation had existed in both eyes—the right had recovered. In the left there was, during the intervals between the attacks, only perception of shadows. The general appearances were those of hereditary syphilis. In both, the papillæ were unusually distinct; they could be seen without the convex lens. The right one was normal, but was surrounded by a brilliant white patch, well defined by a line of pigment between it and the choroidal red: in the left, the same patches appeared; the vessels were tortuous. Further from the papilla were many small patches, some with a dot in the centre. The macula lutea was normal in both eyes. It is remarked, that the patches round the papilla in the left eye were not enough to account for the bad sight. It is probable that some change has taken place in the optic nerve; and it is unusual to see the larger choroidal vessels next to the white patches. In normal eyes, it is only uniformly red. There are no positive evidences of syphilis, but many indirect signs.

Cerebral Amaurosis. The ophthalmoscopical signs are by no means satisfactory.

Acute or Inflammatory Amaurosis. Mr. Hogg, at page 117, notices a case related by Ruete, in which the amaurosis was sudden; the papilla was hazy, and there was over the centre a black spot with a grey ring; there were retinal exudations, with anæmia of the optic-nerve vessels. And, at page 123, he gives a case of amaurosis complicated with inflammation of the posterior tunics, in which the papilla in the left eye was covered by a dark spot which extended as far as the macula lutea; there was also excessive vascularity of the choroid and retina.

Traumatic Amaurosis. Mr. Hogg, at page 117, relates a case of amaurosis from injury; there were found, besides lenticular opacity and some floating bodies in the vitreous, serous effusion on the retina, and beneath it irregular white patches.

Amaurosis with Congestion. Mr. Hogg, at page 57, relates a case in which the patient had lost the faculty of seeing any but large letters, and a bright star seemed to be constantly floating before the eyes; there was no pain or inflammation. The principal appearances in both were great congestion of the retinal vessels; and the centre of the fundus, over the macula lutea, was covered by a small black spot.

Amaurosis with Effusion. Mr. Hogg, at page 58, relates a case where, with symptoms somewhat like those of glaucoma, in the right eye, the papilla was surrounded by an irregular margin and dark lines of pigment; in the left, the portion of the fundus in which most vessels were conspicuous was covered by a grey film.

Amaurosis with Hemiopia. Mr. Hogg, at page 39, notices a case in which this was the most prominent symptom. There was cupping of the papilla; a black patch over the fundus, partly covering the papilla; floating pigmental masses, etc. And, at page 40, he describes another case, in which the retina had become partly detached. At page 41, he quotes one from Anagnostakis, in which floating flocculi were observed in the vitreous humour; and a mass which sank to the bottom when the eye was at rest, which he suggests was the remains, probably, of an old hæmorrhagic clot.

Amaurosis with Anæmia. Mr. Hogg, at page 59, gives a case from Anagnostakis, in which an amaurotic state supervened upon attacks of dysentery. An examination showed much congestion of the retina, with many hæmorrhagic patches.

Amaurosis with Exudation. Mr. Hogg, at page 122, relates a case of amaurosis, partial or temporary, where he found the fundus covered in certain spots with irregular white patches. A pinkish cloud looked as if suspended before the papillæ, which were quite undefined at the margins.

Amaurosis Congenital. Mr. Hogg relates a case at page 141, in which, with congenital defect of sight, there were many signs of want of development of parts, a very pale fundus, and general want of pigment. The papilla looked unusually distinct and white. The vessels were very small.

Amaurosis with Nyctalopia. Mr. Hogg, at page 115, notices a case given by Anagnostakis, where, as a result of injury, nyctalopia came on. He found the vessels of the fundus much congested, and haziness of the tunics from effusion. Near the papillæ were three cretaceous scales, slightly raising the surface.

Myopia, Presbyopia, etc. The defective power of adjustment of the eye to distances, including the affection called asthenopia, may generally be distinguished from amaurosis by negative signs. If we find the fundus and papilla and media in a natural condition, we may conclude that the derangement of vision is caused by loss of this power. Myopia may generally be detected by the ophthalmoscope, by two signs principally: firstly, an unnatural distinctness of the choroid, as well as the retinal circulation, which enables us to see the fundus, often even to trace the papilla, the macula lutea, etc., without the aid of the convex lens; secondly, the myopic arch or crescent, which consists of a bluish white crescent, describing the half or more of a circle, concentric with the margin of the papilla, comprising, at the thickest, about two lines in breadth, often irregular at the margin, and surrounded by a black line or mark, showing accumulation of the pigment. The cause of this appearance in myopic subjects is at present involved in obscurity; but the appearance seems to answer in great measure to that described above, from Desmarres, under the head of "sclero-choroiditis". In both cases, there must be change in the choroidal circulation, and in the distribution of the pigmental elements. The above may be the only ophthalmoscopical signs observable; but, in aggravated cases of myopia, we have others superadded. Dr. Bader, in *Ophthalmic Hospital Reports* for April 1858, states that he has known this crescent, as well as the myopia, disappear in a woman after her bearing a third child; and, in another case, observed it disappearing gradually, losing its crescentic shape, and normal and choroidal red appearing in its place. The fundus seems to become pale red before the spreading of the crescent. In some cases he states that great myopia is present without the crescent, and the only peculiarity is that we can see the fundus, etc., quite clearly without the convex lens, and sometimes we find cases of weak sight with patches similar to those above described, but without myopia, concave glasses giving no benefit.

Asthenopia is the disease, the essential nature of which is inability to keep the attention fixed long on letters, etc., with loss of adjusting power. We commonly find no signs, but Mr. Hogg, at page 110, relates a case where he found the fundus pale. The retinal vessels covered by a greyish web; the vessels

few; anæmic retina, as shown by an unnaturally white papilla.

Glaucoma. There are three principal signs by which we diagnose this affection as regards the ophthalmoscopical characters. Firstly, the optic papilla is altered and cupped. Secondly, the vessels are altered in direction. Thirdly, there is spontaneous pulsation of the arteries. First, cupped papilla. There is a clearer distinction between the papilla and the rest of the fundus than usual; the outer border looks prominent, like a picture frame, and has a yellowish appearance, while the interior has a greenish hue. The choroid in the vicinity shows some maceration of pigment. The vessels at the border are seen to curve over and disappear, and to reappear in a position differing from what they would have if they had proceeded in a straight direction. This is best seen as regards the veins. Then the vessels are seen to have a pulsation isochronous with that of the heart. All, or any one of these signs may be present. As regards the pulsation, it is to be observed that spontaneous pulsation is observable in some other eye affections, of which amaurosis is the result; and even in the healthy eye of persons in whom general pulsation and muscular action are unusually strong. We can also often produce it by pressing upon the globe while we are observing with the ophthalmoscope. If we press the globe until we distinctly see the arteries pulsate, we shall find the patient's vision diminish from the circumference inwards, until it ceases altogether. This fact may give us some measure of the amount of deterioration of vision following the changes which occur from pressure in glaucoma; but in some cases, where we find a great amount of compression exerted, the vision is not proportionately affected. Mr. Hogg, at page 59, notices a case of glaucoma in which the principal appearances were great congestion of the fundus, with œdema of the retina; the papilla was discoloured by pigment deposit. Also he mentions other cases in which there were signs of bloody effusion, or simply hyperæmia of the retina. For information on the subject of glaucoma, the reader is referred to articles in the *Ophthalmic Hospital Reports*, by Critchett, Bader, etc.; in the *Medical Times and Gazette*, by Hulke; also reports of a paper on the subject read by Mr. Hulke before the Royal Medical and Chirurgical Society; etc., etc.

[*To be continued.*]

Transactions of Branches.

BATH AND BRISTOL BRANCH.

PRESIDENT'S ADDRESS.

By JAMES GEORGE DAVEY, M.D., Northwoods.

[*Delivered July 14th*, 1859.]

GENTLEMEN,—When seven years since I became a member of the Bath and Bristol Branch of the British Medical Association, I did not anticipate—nor had I any right so to do—the proud position I have the honour to hold this day. My best thanks are due to you for having elected me your President during this present year. It will be my endeavour to deserve the confidence you have, to all appearance, reposed in me; and not the less to promote, during my year of office, the interests of medical science, and those especially of our own important Branch. I confess to you I feel not a little diffident of myself, and of my abilities to do credit to the office you have elected me to fill. I am aware that I succeed many excellent and accomplished men, and that comparisons may be looked for as between them and myself; comparisons which may not unlikely reflect indifferently on my own endeavours to gain your satisfaction: however, I promise you to do my best, at all times, and under all circumstances; and in doing this, I doubt not I may rely on your kindly sympathies and support.

Gentlemen, within the walls of this institution you have listened to addresses from many with whom I may not venture to compete; you will, therefore, not expect to find in my address this day anything startling or original, such as has emanated, in times now gone by, from such men as the late Dr. Prichard and Dr. Carpenter. These walls have, as you are aware, echoed their thoughts, resounded the force and beauty of their sentiments; thoughts and sentiments which have, doubtless, not been without their good effects on the youthful and painstaking among the audience occupying the benches whereon you now sit. The lessons here taught by Drs. Prichard and Carpenter have, in all probability, borne fruit; their labours in the great cause of truth have not been without good results.

Standing thus in the area once occupied by these illustrious men, I will do my best to imitate the means whereby their successes were achieved; and to follow on this occasion in their footsteps.

The present is an opportunity of which I will avail myself to make a few remarks on the medical calling, on our duties to ourselves, and to the public generally. It occurs to me that our profession is, at length, taking a start in the right direction; that individually we are beginning to appreciate the position we occupy as members of a learned body; that our universities, and colleges, and halls, are opening their senses to a new and brighter view of things, and that the public sympathy is more than ever inclined to go with us. Experience seems to have done much for us and for our institutions; it would appear to have led us to perceive the real character of our mission, as members of the social body; it has taught us to realize the fact of our having duties to perform, both towards ourselves and towards our neighbours, *i.e.*, society. Truly this experience has been dearly bought; but, on this account, is it not the more valuable? I think so. How recently have we had evidence of the beneficial results of this same experience! Within the space of a year or two, we have seen the Colleges of Physicians and of Surgeons, both of London and of Edinburgh, acknowledge the error of their past ways, and adopt a better system—one of a liberal and progressive character. We have seen, too, the Provincial Medical and Surgical Association struggle through its brief and abnormal being, and come out of its chronic and almost fatal asphyxia—an asphyxia at the heart-core; the result, mind you, of an attempt to fasten the old and the obsolete to the last new fashion; in other words, to amalgamate the conservative tendencies of a bygone day with the newest inventions and the most modern requirements. The existence of the British Medical Association, as we see it now, demonstrates to us, though on a small and *medical* scale, the fact that no mere party or exclusive government of a body of men, be they doctors or what not, provided they are of the Anglo-Saxon race, can at this day prevail; and for this simple reason, that exclusive government strikes at the root of all justice and self-respect; it excites an unnatural or morbid relationship between the governing and the governed.

On these grounds, I say, let a part of our mission be to well organise ourselves; to place ourselves in the most favourable position as members of a learned and honourable calling.

Young men of good acquirements, and of honourable views of life, are more than likely to be well disposed and attracted towards a profession like ours, when they hear and perceive that our corporate bodies, or colleges, and associations, rank high—*i.e.*, embrace in their forms of government whatever is just and reasonable, and take no cognisance of *cliqueism*. Such young men it is who will do honour to the medical profession, both in a moral and in an intellectual sense; and such young men it is on whom we must depend for our Harveys, and Jenners, our Thompsons, Livingstones, and Owens;—names these, never to be forgotten; names these, the mention of any one of which is sufficient to assure us that both science and humanity, even religion, are more than safe in our keeping. Each one of the five named was or is a *hero*, in Carlyle's acceptation of the word; each one had or has the fire of genius within, prompting him to the leadership among even the able and the good.

Whether we contemplate the learning of the great Harvey, or the patient research of the immortal Jenner, or the heroism of Thompson on the battle field, or the enthusiasm and courage of Livingstone in the wilds of Africa, or the very able and successful investigations of Owen, we are led, as it were, irresistibly to realise this plain matter of fact so well embodied by the poet—

> "'Tis in the advance of individual minds
> That the slow crowd should ground their expectation
> Eventually to follow—as the sea
> Waits ages in its bed, till some one wave
> Out of the multitude aspires, extends
> The empire of the whole, some feet perhaps,
> Over the strip of sand which could confine
> Its fellows so long time: thenceforth the rest,
> Even to the meanest, hurry in at once,
> And so much is clear gained."

Here is good reason then why we should do what in us lies to render our calling attractive to the youthful and ardent, who are seeking a position of usefulness and honour in the world.

The alterations and improvements which have, of late, grown into existence, in the constitution and government of

our Colleges of Physicians and Surgeons, and of the British Medical Association, the recent acceptance by the profession and the public of a Medical Reform Act, etc., will, it is to be hoped, have the effect of diminishing, if not of removing, the jealousies and disunions occurring among us. Let us hope that the more liberal spirit which pervades the amended laws and regulations of our corporate bodies will not be without its good effects on individual members of the medical profession. Let us hope that *unity* will now more than ever pervade the several ranks of medical practitioners, as it does the various departments of medical science, which we together pursue.

Now I have suggested that *our mission may be said to be two-fold.* The first part of it involves the recognition of certain duties as concerns ourselves. If any one man would discharge his responsibilities to the public satisfactorily, and with advantage alike to himself and to them, he must not fail to do what will preserve his own health intact. The fear is, that now-a-days, in too many instances, medical men are led on too far with the ambition of being useful to their fellow man. On the eve of life, full of hope and confident of success, with apparently sound health, a vigorous body, and an active mind, what has not the future promised to some of us? Many an aspirant for public honours, many a zealous and warm heart, many a bright and glorious intellect, have been wrecked, stranded on the fatal beach, ere they had well floated on the deep waters. Ardent in the pursuit of truth, eager in doing good, never resting in the pursuit of medical knowledge and bed-side experience; many a good man heeds not the small and oft-repeated indications of functional derangement, of indigestion or nervous irritability; on, on he goes; work, work. His calls are unceasing; his duties endless; day by day he draws freely and yet more freely on his overstrained nervous system. There must be and is an end to this perpetual infringement of the organic laws. He leaves home for a short period; but is soon at work again. It is the old story over and over again. An immense practice, great success, unbounded popularity, have been the *rewards* (if such, under the circumstances, they can be called) for exertions as untiring as they were honourably and successfully directed towards the relief of bodily suffering, the alleviation of human woe, and the cultivation of science and knowledge. Apart from all mere *self* in this matter of worldly progress, the "rewards" here glanced at, however ill-timed, are as nothing in comparison with the public good effected, in comparison with the great results of intellect trained to purposes of utility and progress, and of principles of virtue, otherwise innate, drawn into activity, and made to diffuse their blessings through the family circle.

But herein lies the pity: the "hero of my tale," though he may have originated the most unexceptionable local *hygiene*—though he may have created the most desirable sanitary schemes, and improved the minds and bodies of his townspeople, and taught them the wherewithal to honour nature and love her behests—succumbs, ere long to his self-imposed labours: his mental powers decline, and his scenes of usefulness become but a myth. The "nervous irritability," of which mention was just now made, robs him of all normal volition; and, as a consequence, he—in pity be it said—no longer realises the *mens sana in corpore sano.* Gentlemen, I am sorry to say I am drawing no imaginary picture: I am alluding to a dear and departed friend, for many years a honoured and honourable member of our profession: I mean the late Dr. Engledue, of Portsmouth; one cast in the best mould, combining in himself the best qualities of our common nature, and, despite of certain peculiarities, realising all the requisites of a good man. His career should not be without its advantages to us living; he, in the ardour of his feelings, and the earnestness of his pursuits, day by day, seemed to forget that—

> ——— "Knowledge is as food, and needs no less
> Her temperance over appetite, to know
> In measure what the mind may well contain;
> Oppresses else with surfeit, and soon turns
> Wisdom to folly, as nourishment to wind."

One word more—the gentleman named to you, after a confinement of more than two or three weeks to his house—and whilst suffering from what was supposed to be "hypochondriasis," was found lying on his bed in the struggles of death, and close beside him was found a small bottle labelled "prussic acid." The fact here stated I would have you bear well in mind, because it would appear probable that a valuable life might, in this instance, have been saved, had the necessary precautions been observed. The death, however, of this "public benefactor" diminishes our number by one, and of this one we are advised, by an elegant writer in the *Portsmouth Times* of December last, to—

> "Look not coldly on his clay,
> Turn not shudderingly away;
> Though about it hangs the gloom,
> Of a sad, a self sought doom.
>
> "Think on all the gifts and grace
> That had here a dwelling place:
> The large heart, the spirit brave,
> All that he to others gave.
> Think upon the life that's gone,
> Crowded with its work well done;
> Precious life—too freely spent,
> Till the o'erwrought spirit bent
> 'Neath its weight of toil and care,
> Yielding darkly to despair.
>
> "As ye gaze upon the face,
> And the spirit anguish trace,
> Of a life, though bravely past,
> Wreck'd and blighted at the last,
> Weep—for life may pass from thee,
> Ere his fellow thou shalt see."

The observations already made relate to ourselves, either in a corporate or in an individual sense; the most important part of our mission remains to be considered. The relations of the medical profession to society at large have, within a comparatively short time, become much modified. Our functions were, at one time, bounded within much narrower limits than now. Doctors were expected only to cure disease; or, if not to cure it, to relieve or mitigate the sufferings consequent on the same. At this day, however, our relations to society have assumed a more practical and imposing aspect. The legislature and the public demand from us the means whereby disease can be prevented. The growing intelligence of the age has removed a very considerable amount of that mere superstitious feeling, which led pretty nearly all classes of the human family to refer the occurrence of a fever, or an attack of dysentery, or other disease, to the direct interposition of the Divine Will, as if nature governed not by general, but by vain and partial laws; and the consequence has been that, throughout the length and breadth of the land, in high and in low places, among ministers, members of parliament, poor-law boards, and even the commonest parish officials, there is manifested an earnest desire to get at a knowledge of hygienic medicine. The Registrar-General and his coadjutors, the various health-officers, demonstrate in their reports to how great an extent the lives and the health of mankind depend on the due observance of the organic laws, and in what manner the prevalence of endemic and epidemic disorders results from the neglect, among us, of either an efficient drainage, a due ventilation, or the habits of cleanliness, etc. Thanks to the labours of Dr. Southwood Smith, Arnott, Mayo, Caldwell, and others, sanitary science has become an integral part of medicine; its practical recognition by the state, and in our large towns, will result in the improvement of the dwellings of the poor, and in the realisation of the best endeavours of that excellent man, the Earl of Carlisle, to obtain the enactment of the Nuisances Removal and Diseases Prevention Acts. If ever mankind shall live in the obedience of the natural laws, if that time ever arrives when man shall be found harmonising his daily routine with the requirements of his organism, adapting himself, to use the words of the late George Combe, to "external objects," *i.e.*, to the physical, intellectual, and moral laws, then will our profession have brought to a successful and honourable issue its all-wise endeavours to civilise man, to encourage him forwards in the course of his amelioration, or, if you prefer it, to realise its second, or, as I have termed it, the most important part of its high mission, as it concerns our fellow man. I would impress this "sanitary question" on your consideration; I would ask you as men and philanthropists, to give it your best attentions. Mr. Edwin James, and even Lord Shaftesbury, and Mr. Walpole too, good, but in some respects deluded men, may sneer as they will at us, may doubt our intentions, misconstrue our motives, and make the very best of every opportunity to throw doubt and suspicion on us; but, as a body, we are proof against their attacks. I hesitate not to say, that we fear not their invectives; and, what is more, that we are prepared at any time to bear the issue of a fair comparison in the matter, of not only intellect, but of high moral worth and integrity, with either lawyers, courtiers, or statesmen.

I have just now mentioned the names of S. Smith, Mayo, and Caldwell, as foremost among those of us who have led the public to appreciate hygienic medicine; it would be unjust in me were I not to add the name of Simon to the above. With his able writings on this subject, you are all familiar; but you

will, perhaps, permit me to illustrate Mr. Simon's earnestness in this good cause, by quoting the following lines from a late report of his; their force, eloquence, and practical bearing will, I doubt not, render them especially acceptable to you.

"It is, indeed, only by very gradual increase that legislation and Government may succeed in giving to human life the same security against the infliction of preventible disease as against the infliction of wilful violence; and millions will have died before this public carefulness for individual safety can have become co-extensive with even the present certainties of preventive medicine. But growing knowledge must bear its fruit. It has now been fully recognised that within the very centres of civilisation controllable influences are working against human life more cruelly than brute violence ever worked in the first discordant beginnings of society. It has been shown that in certain districts of England, the operation of those controllable causes is vastly more powerful than in others; that, within the rule of certain sanitary authorities, particular forms of disease undergo a multiplication—a fivefold and tenfold and hundredfold multiplication—of their lowest familiar fatality. To suppose that such sanitary authorities could permanently disavow an interest in this knowledge, or that public opinion could long hold them irresponsible for so monstrous a waste of life, would be to misunderstand the meaning of civilisation, or to belie the humanity of England."

But it is not the prevention of disease only that constitutes the claims of the medical profession on the public sympathy and consideration. Need I add that "the sanitary question," so called, takes an especial cognisance of matters educational; that it recognises the principles whereby both the intellectual and moral qualities of our being are developed and brought into a normal activity; that it embraces the means necessary to foster and encourage, in the youth of both sexes, each latent knowledge, and virtue, and good quality of mind—and without which means such in great part must stagnate, and so die in the rising generation. Need I remind you that, whatever may be the physical superiority and prowess of the Anglo-Saxon race, whatever may be the physique and great bodily endurance of our fellow man, however square his shoulders, and rounded his form, and plump his muscles—these several requisites of brave men and true are but the effects of a sufficient physical education—of, in point of fact, good food, cleanliness, pure air, sufficient clothing, and temperance in all things mental and corporeal.

On these grounds, then, we are led to perceive the basis on which we, as a profession, stand, and to understand how it is that we are really the pioneers of a sound and enduring civilisation.

If the facts of medical science be ever appreciated as they deserve in the matter of education; if the individual peculiarities of thought, and feeling, and desire, in children and youth, receive the attention they demand; and if then the vicious routine which at this time obtains is consigned to the oblivion which even now "looms in the future",—then will certain of our medical brothers come in for a share of their deserts. Gall and Spurzheim will one day have their rewards: to these especially will all honour be given. The late Dr. Andrew Combe and Dr. Caldwell, Dr. Elliotson, Dr. Conolly, not forgetting the late Dr. Engledue, and the eminent professor of medicine at Edinburgh, Dr. Laycock, have each of them taught the psychology which Gall originated, and to which Spurzheim devoted the best energies of an active and noble mind. Phrenology is not agitated now-a-days; it is an accepted fact by all who have cared to investigate it. It is of no avail that a few good men, not understanding the thing, decline to accept the revelations of Gall. The fact is, the objectors alluded to have theories of their own: their "self-esteem", then, is in the way of the great truth insisted on. Drs. Carpenter and Bennett are the authors of certain speculations concerning mind and matter which, I hesitate not to affirm, are altogether unsound, and unworthy of their high and well deserved reputations. Now that I am on this subject, permit me to recommend to your attention a well written article in the April number of the *Psychological Journal* of Dr. Forbes Winslow; it is called, "On the Artificial Production of Stupidity in Schools". Whilst giving its author credit for the best intentions, it occurs to me that even *he* has much to learn of the "nervous centres". He commends very properly the "painstaking researches of Bell and M. Hall"; but he says not a word of Gall, the very foremost man of all who have ever written on these same "nervous centres". It was Gall who started the late Sir C. Bell into being, so to speak; it was Gall, if it was not Walker and Johnstone, who gave the late Dr. M. Hall his first ideas respecting the excito-motory system. Moreover, it was Gall who, in the words of the author of the paper above named, demonstrated for the first time in 1810, that "*the sensational and intellectual functions of the human brain are not only distinct, but also in some degree antagonistic, through the application of the ordinary law of nutrition to their respective organs*". Lest you might conceive that I am straining the point, I will read to you Mr. Grainger's opinion of Gall, as expressed in the following few words at p. 22 of his work *On the Spinal Cord*:—"The true anatomy of the cerebrum was perfectly unknown till the researches of Gall; and it is due to the character of this eminent man, and of his pupil, Spurzheim, to state that all our knowledge of the anatomy of both the brain and the spinal cord has resulted from their inspections; and, even with respect to those splendid discoveries" [Mr. Grainger is here alluding to those of Gilbert Blane, Mayo, Müller, and Marshall Hall] "which have thrown so much light on the functions of the nervous system, I believe that most, if not all of them, may be traced to the same source." Yes; Gall it was who demonstrated how and in what manner "the character of man is formed for him, and not by him"; how it comes "that minds *are* different even at birth, and require, therefore, as different a treatment as Iceland moss from cactuses, or bull-dogs from bull-finches", to quote the telling remark of Martin F. Tupper. (See *The Twins: A Domestic Tale.*) It was Gall who demonstrated for the first time this great fact—that the moral qualities of our being, and our several passions, whether good, bad, or indifferent, are found in direct connexion with, and strictly dependent on, our material or physical nature. He was thus prepared to understand the real motives to action in man, and thus to get at the fallacies of our criminal laws. The means of prevention of crime, the treatment of criminals, and gaol discipline and government, became in his hands a *system;* the empiricism which had so long governed these several matters was rendered obsolete by him. Gall deserves, doubtless, to be regarded a pioneer of a sound and enduring civilisation, as one among our body who has failed not to realise his high mission, and not the less to prove that the medical profession has a strong claim on the public sympathy.

You are aware that Hippocrates was once sent for by the people of Abdera to visit Democritus. His "exuberance of good", we are told, "had caused a disease"; and this so excited the sympathies and alarms of the Abderites that they sought the advice of the most famous physician of that day, fearing lest he who so "elevated the city" should become mad; and thus, said the Abderites, "our good city of Abdera will be desolate". As an encouragement to Hippocrates to come quickly, the fellow-citizens of Democritus promised the most abundant reward. They told Hippocrates, "If our city were all gold, we would give it you to restore this most excellent person to health." "Thou wilt not, oh Hippocrates, cure a man, but a city, a languishing senate, and prevent its dissolution; thus becoming our lawgiver, judge, magistrate, and preserver." Is it not refreshing to learn this much of Democritus and the Abderites? That he laboured well and successfully for the public weal, cannot well be doubted. When, some two months since, I strolled through Trafalgar Square, and was led to admire the statue of Jenner, and to contemplate the circumstances attending his bright career, my thoughts reverted to Democritus and the senate and people of Abdera. That both Democritus and Jenner were public benefactors, is certain; that they well earned their celebrity, cannot be denied. Now comparisons, they say, are odious; but the locality was suggestive. I asked myself what the generations yet to come would think of Jenner side by side with Nelson and Napier—the medical philosopher, the public benefactor, side by side with fighting men—with the heroes of Trafalgar and Scinde. Is not the one for all time—the others named but for a period? The name of Jenner will, in the long time yet to come, be associated only with a sound and progressive civilisation; the names of Nelson and Napier, with but blood and rapine. Our hearts are, I doubt not, in the right place. The presence of the statue of Jenner in a metropolitan square indicates the direction to which the sympathies and earnestness of the great public tend. As Democritus was to the Abderites, so, it may be said, is the memory of Jenner to the British public. Time will doubtless multiply cases parallel to these.

I had intended to dwell on two or three other topics; but time so presses, that their mere mention must suffice.

The medical profession should be well represented in the British Parliament. The public would have benefited, long years since, by the Vaccination Act, the Health of Towns Act, and the Medical Act, etc., and ourselves would have

profited thereby, had those two ominous letters—M.P.—appeared less seldom among the honours of our class. Let me ask, Ought not the British Medical Association, now that it is really a national affair, to claim to return at least two of its members to Parliament? If it should ever fall to my lot to hold a seat on its Council, I will most assuredly agitate this question. If antiquated Colleges, with a fixed locality, should have their especial M.P.'s, surely modern and national associations may claim as much. I would ask you, gentlemen, to remember this suggestion.

Another important item I would offer to your consideration. The curiosities of medical literature are neither few nor far between, and these not infrequently involve more or less personal feeling and dispute. It is not a little difficult sometimes to name a writer in whom has originated a certain and recognised fact in medical science. Now, it has occurred to me that a sub-committee of the British Medical Association could be well employed in compiling a registration-table, wherein the names of discoverers, the dates of their labours, and the specifications or descriptions of such labours, with their results, etc., should be set down and chronicled with all accuracy and good faith. My reasons for offering this suggestion shall be as brief as possible. Hardly one year before the death of Dr. M. Hall, he addressed, in 1857, a letter to the editor of the *Lancet*, in which he announced, as a recent discovery of his own, the existence of the "excito-secretory system". "Henceforth," writes Dr. M. Hall, "the diastaltic nervous system must be divided into two sub-systems—1. The excito-motor; 2. The excito-secretory." But, gentlemen, it is a matter of fact that Dr. Campbell, of the United States, not only taught this same doctrine, but gave it the same name as Dr. Marshall Hall; and this so long back as 1853. Dr. Campbell, then, anticipated Dr. M. Hall by four years. (See Dr. Campbell's *Contributions*, 1857.) Nor is this all, as concerns the excito-secretory system. I am sorry, in some respects, to do or say what might tend to disappointment in one so accomplished and original as Dr. Campbell; but truth will out, in some way and at some time. At pages 138–143 of Mr. Grainger's work *On the Spinal Cord*, you will find the excito-secretory system explained in detail, and its integrity referred to certain "incident" and "reflex" filaments of the sympathetic nerve. Mr. Grainger refers, for confirmation of this doctrine, to "the flow of saliva during mastication", to the action of "sialogogues", and to the "regulation of the supply of the gastric juice." One word more. In the January number of the *Encyclopædia Britannica*, in the article "Physiology", from the pen of Dr. J. Hughes Bennett, Dr. Campbell, of the United States, is mentioned in such a way as to lead the reader to infer that the idea of the "excito-secretory system" of nerves originated in him (Dr. Campbell). The same gentleman has given to the world certain views of the pathology of "typhoidal fever"; and these, too, he conceives original. In the columns of the *Lancet* for 1836, he will perceive how much he has been anticipated by myself.

Gentlemen, it is possible to multiply facts of the above nature to an extent well calculated to astonish those who have not sought for such. A comprehensive arrangement of the chief successes of medical writers would be attended with much good. The fact is admitted, and on good authority, in so far as sheer practice is concerned. Those of you who enjoyed, as I did last year, the high privilege of listening to the able and eloquent Address in Surgery by Professor Miller of Edinburgh, will remember what he said in reference to this point. His words are:—"Arrange as you will, it may be said, there will be jostling. Not necessarily. The lines may converge and meet, but need not cross; and as they meet, the contact need be none other than friendly; proving, indeed, the source of mutual help and encouragement. In such a crowded fleet as ours, there is, no doubt, some risk of confusion and entanglement, more especially if it come on to *blow*. But let each several ship sail *straight* in all weathers, keeping its defined course; let the compass be true, and the steering steady; there will be no collision. Or if, notwithstanding all our care, there be now and then a fouling, yet, with good temper and skilful seamanship, we may boom off and make all clear again, the golden rule working thereto with an almost mathematical exactitude. 'Let A. do to B. as he would have B. to do to him, and the product will give the rule of conduct required.'" To fix so "golden" a rule on the mind; to realise so heavenward a thought; to give, as it were, emphasis to a "rule of conduct" so much beyond all praise,—permit me to conclude this address with the following few and highly appropriate lines:—

"Self-love but serves the virtuous mind to wake,
As the small pebble stirs the peaceful lake;
The centre moved, a circle straight succeeds,
Another still, and still another spreads;
Friend, parent, neighbour, first it will embrace;
His country next; and next all human race;
Wide and more wide th' o'erflowings of the mind
Take ev'ry creature in of every kind;
Earth smiles around, with boundless bounty blest;
And heav'n beholds its image in his breast."

MIDLAND BRANCH.

CASE OF PNEUMOTHORAX, SUCCESSFULLY TREATED BY PARACENTESIS THORACIS.

By S. H. Evans, Esq., Derby.

[*Read June 23rd*, 1859.]

Miss E. L., of delicate constitution and marked tubercular diathesis, eleven years ago had repeated attacks of hæmoptysis and cough, followed by expectoration of thick mucus, with the usual physical signs of a small vomica in the apex of the right lung. After some months, the pulmonary symptoms subsided; the health was much improved; and there seemed to be an arrest of the progress of the disease for a long time. She did not, however, recover her usual strength. During the seven years which followed, she enjoyed moderate health, with the exception of occasional attacks of cough, especially during the winter months.

On July 20th, 1856, she walked about five miles, and felt more than usually well on the evening of that day. She retired to bed in her usual health; but, soon after lying down, she felt a sense of choking, and could not breathe freely. She had no pain, and slept tolerably; but the next two days the breathing became gradually more difficult.

July 23rd. The oppression of breathing had so much increased, that she could not lie down; and, on attempting to do so, she experienced a sense of impending suffocation. When I was called to her at midnight, the respiration was laborious, short, and frequent, forty and upwards in a minute. Pulse 120. On the slightest movement of the body, she was threatened with suffocation. The sudden occurrence of these symptoms led me to suspect perforation of the lung. I examined the chest, and found the right side much distended and larger than the left; the sound on percussion on the right side was tympanitic: respiratory sounds were absent; there was no amphoric breathing nor metallic tinkling. On the left side the breathing was normal. The conclusion I arrived at was, that there existed a large accumulation of air in the pleura. Palliative means were used, but without much benefit. Being unable to lie down, she remained in the sitting posture night and day, but could not sleep.

The symptoms became gradually aggravated, until July 28th, when she appeared *in extremis;* the distress and labour of breathing were almost indescribable; at each breath she uttered a plaintive wail. She was greatly exhausted, not having lain down or obtained more than a few minutes sleep at a time since the 23rd. Her legs, hands, and face, were swollen, and of a livid colour. She was incoherent most of the night. I proposed paracentesis thoracis, as the only means of saving her from immediate death; and, after consultation with Mr. W. Johnson, who agreed with me as to the propriety of the measure, I passed a trocar between the sixth and seventh ribs into the right pleural cavity, and there escaped through the cannula a large quantity of air, which afforded immediate and very great relief. The air issued with great force at first; but as soon as the inspirations became more free, I removed the cannula, lest air should be drawn into the chest from without. I closed the wound with plaster; and my patient was placed in bed in a half recumbent posture. Her breathing was still quick and short, but comparatively easy; and she complained only of a slight soreness in her side.

July 29th. She remained in the half recumbent posture; breathed more freely. The side was stiff and sore, and there was extensive subcutaneous emphysema over the whole of the right side and back. Percussion of the side was clear, but no longer tympanitic; respiratory sounds could be freely heard over the whole lung. Pulse 100.

From this time her recovery progressed without interruption. The return of vesicular breathing in the collapsed lung was gradual; it improved daily, but it was nearly a fortnight before it could be said to be in a normal state. The recumbent posture was not attempted for several days after the operation, and it was some weeks before the patient could lie

comfortably on the right side. The subcutaneous emphysema slowly subsided, and disappeared in about ten days. About a month after the operation, there remained no unfavourable symptoms; and she thought herself better than she had been for some months.

It is nearly three years since the operation was performed, and nothing has occurred to invalidate its success. Her health, always delicate, is now as good as it has been for years. She has slight cough, and sometimes expectorates blood; the symptoms are of a very chronic kind, yet they sufficiently mark the presence of tubercular mischief in the lungs; the physical signs also indicate circumscribed disease in the upper and anterior part of the right lung, at which part percussion is dull, respiration bronchial, and with slight mucous *râle*. Posteriorly, respiration and percussion are equally good on both sides. There is no collapse of the thoracic parietes on the right side of the chest; admeasurement being the same on both sides.

Remarks. The rational and physical signs were well marked in this case, and afforded strong presumptive proofs of the presence of a large accumulation of air in the right pleural cavity. The slow and gradual increase of the symptoms of pneumathorax, which commenced rather insidiously on July 20th, and advanced slowly without occasioning urgent dyspnœa until the evening of the 23rd, favoured the supposition that the opening through which the air escaped was of small size, and it is probable that the aperture became obliterated by lymph previous to the operation of tapping the chest, and possibly prior to my visit on the 23rd, as no respiratory sounds could be heard either at that time or afterwards, until the 29th.

The successful termination of this case clearly proves the advantage of tapping the chest in pneumothorax, when the accumulation is so great as to cause threatening suffocation.

Two similar cases are mentioned by Dr. Copland; and I shall conclude these remarks by quoting the following, which he relates at page 311 of his *Dictionary*, and which, in respect to the severity of the symptoms, bears a strong resemblance to this case.

"In 1833, a case of pneumothorax occurred in my practice, at an early stage of phthisis, the patient being young and robust, and not having lost flesh or strength. The nature of the mischief was prominently characterised; the heart was pushed remarkably far to the right side, and the rational and physical signs were all marked and extreme. The dyspnœa and distress were great. I advised that an opening should be made for the exit of the air, and the friends of the patient desired that Sir A. Cooper should make it. We met a few hours afterwards; he admitted his ignorance of the nature of the lesion. I fully explained the cause of the symptoms and of their extreme urgency; but he refused to perform the operation, stated that he never had performed it with the view of letting out air, and that he would not now do anything so novel, although he would have been ready to undertake it when he was a much younger man. Having heard Sir A. Cooper's determination, the patient's friends would not allow the operation to be resorted to by any one else, and the patient died asphyxied a few hours afterwards. The operation, if it had been resorted to in this case, might have prolonged life for a very considerable period."

CASE OF WOUND OF THE CORNEA, SCLEROTIC, AND IRIS, FROM WHICH AN IRON SCREW WAS SUBSEQUENTLY EXTRACTED.

By J. Wright Baker, Esq., Derby.

[*Read June 23rd*, 1859.]

On the evening of March 18th, 1858, a deaf and dumb lad, aged 18, was brought to me, having sustained a severe injury to the left eye. Whilst working at a screw-cutting machine, in a manufactory in this town, he had been struck on the eye with great force by a fragment of iron, which produced a large wound and instant blindness. On examining the eye, the wound was found to extend through the lower segment of the cornea and a portion of the sclerotic, and measured from four to five lines in length. The aqueous humour freely escaped, and with it a little of the vitreous humour. The iris also was wounded and protruding, the pupil being no longer visible; a free extravasation of blood having taken place, so as to obscure the anterior chamber.

I at once applied the extract of belladonna freely all round the orbit, prescribed two grains of calomel and half a grain of opium in the form of pill, every four hours, together with a saline mixture; and ordered cold water dressing to be kept constantly applied.

Notwithstanding the violence of the injury inflicted, comparatively little inflammatory action followed. This, I think, must be accounted for partly by prompt and active treatment, and partly by the fact that the lad, whose parents were in great poverty, had a half-starved appearance, and a frame debilitated by severe small-pox in childhood. A very short time sufficed to show that further antiphlogistic measures would not be required, and therefore the treatment was changed for quinine and steel, with good diet so far as it could be procured.

In a few days from the time of the accident, the body of the patient became covered with the ecchymosed spots of purpura hæmorrhagica; and, the general cachectic state of the constitution now giving cause for as much anxiety as the eye itself, I yielded to the request of Josiah Lewis, Esq., of this town (who had always taken an active interest in the boy), to have him removed to the ophthalmic wards of St. Mary's Hospital, London, and placed under the care of Mr. White Cooper; feeling assured that, in addition to the professional treatment of so eminent a surgeon, no small advantage would be gained by the patient from the superior comfort and good diet he would there receive. Nor in this was I disappointed; for in the course of time, as I was informed by Mr. Cooper, who kindly wrote to me twice, and to whose courtesy I am indebted for the further history of the case, the health greatly improved, the eye went on favourably, pain subsided, and the wound steadily progressed towards cicatrisation.

At the time the patient left me for London, the globe was considerably diminished in size; the wound had an ulcerated appearance, and was covered with pus of an unhealthy kind. The iris was diminished in circumference, and of a dull green cast; the pupil closed, and firmly adherent to the wound. The surface of the sclerotic was of a dull red colour.

On Saturday, May 8th, six weeks from the time of the accident, and three weeks after the patient's removal to London, Mr. Cooper observed that a dark brown point had made its appearance in the wound, and expressed his opinion that it was metallic, which a touch with the probe proved to be the case. He tried to remove it with forceps, but, finding it to be firmly impacted in the eye, proceeded to extract it under chloroform. Having made an incision through the cornea and iris in their whole breadth, he seized the foreign body, and with some difficulty withdrew it. It proved to be a piece of iron screw, four lines in length by three in breadth, and weighing twelve grains. Its edges were extremely jagged and sharp, and the surface irregular. Although some violence was inflicted on the eye by the withdrawal of this mass, scarcely any irritation followed; and in four days the patient was able to leave his bed and walk about the ward.

When he returned to Derby, I found the globe greatly diminished in size, much collapsed, and with a somewhat irregular surface. The cornea was considerably diminished in size, and showed but little opacity. The general health was much improved.

Remarks. It is evident that, at the time of the accident, the fragment of iron cut through the cornea and iris, and buried itself out of sight in the posterior part of the eyeball. It is indeed remarkable that such a rough irregular mass, so calculated to excite irritation, should have caused so little disturbance that even pain should have subsided, and the wound cicatrised. There can, I think, be no doubt that it would subsequently have made its presence known there; and sympathetic inflammation and destruction of the other eye would have been the probable consequence.

This case points out very distinctly how impossible it often is to ascertain the full extent of injuries to the eye in the first instance; how necessary it is to watch the progress of symptoms, and to seize the earliest opportunity of removing any foreign body from the globe as soon as detected; at the same time abstaining from any premature attempt at the discovery of such body. It also well shows the importance of constitutional tonic remedies in the treatment even of traumatic inflammation of the eye, when judiciously applied; as also of other forms of inflammation in debilitated constitutions.

In conclusion, I will briefly allude to an interesting case described by Mr. Critchett, for a more fully detailed account of which I must refer my readers to the *Lancet* of April 1st, 1854.

In Mr. Critchett's case, an engineer was struck on the eye by a piece of metal, which penetrated far enough to be found lying on the hyaloid membrane, as ascertained during the pro-

cess of extraction. The crystalline lens had been wounded, producing an incipient traumatic cataract. The operator scooped away the greater part of the lens by the scoop of a curette, until the foreign body was brought into view, and by the same instrument removed. Here a good result followed, the inflammatory symptoms being slight, and a tolerable amount of useful vision being ultimately obtained.

LETTERS AND COMMUNICATIONS.

Letters or communications for the Journal should be addressed to Dr. Wynter, Coleherne Court, Old Brompton, S.W.

Letters regarding the business department of the Journal, and corrected proofs, should be sent to 37, Great Queen Street, Lincoln's Inn Fields, W.C.

A List of the Members of the British Medical Association will shortly be published in the Journal. Gentlemen whose designations or addresses, as given in the lists published on July 24th and October 9th, 1858, and June 18th, 1859, are inaccurate, will oblige by at once forwarding the necessary corrections to Dr. Williams, Worcester, or Dr. Henry, 37, Great Queen Street, London, W.C.

British Medical Journal.

SATURDAY, AUGUST 27th, 1859.

THE TRIAL OF THOMAS SMETHURST.

We question if even the trial of Palmer so profoundly moved the public, as that of the unhappy man who now awaits his fate in Horsemonger Lane gaol. Within four years no fewer than four persons in the middle rank of life have stood at the bar, charged with procuring the death of their victims by the subtle introduction of poison into the medicines and food given by direction of the regular medical adviser. Palmer, in 1856, was found guilty of substituting pills containing strychnine for those of a harmless character, and thus bringing about the death of his intimate friend. Dove, shortly afterwards, following his subtle teaching, slowly poisoned his wife in the same diabolical manner, and shared a like fate. Wooler barely escaped with his life from the charge of putting arsenic into the medicines administered to his wife; and here is a fourth case, in which all the skill of medicine was presumed to have been brought to bear, not to save what should have been a very dear life, but so to destroy it that the blame should fall upon nature alone. It was not to be wondered at that, under such circumstances, the prisoner should have entered upon his trial with an extraordinary amount of prejudice against him; and this prejudice was to a certain extent aggravated by his very vile antecedents, which unquestionably prove that he is a man with whom no honest mind can feel the least sympathy—a liar, a cheat, a scoundrel of the blackest dye in every relation of life. Can it be wondered at, that there was a proclivity in the public mind to find him guilty of the still heavier charge of murder?

Whether the verdict pronounced against him be a just one or not, the Great Judge alone knows. The doubt in the public mind on this point is so great, that no one has a right to speak dogmatically; but we do not think we err in stating that, as far as the medical evidence goes, the profession have made up their minds that it is not sufficient to prove the charge against him. If it can be shewn, as we think it reasonably can, that natural causes may have brought about a similar train of symptoms to those which the unfortunate Isabella Bankes suffered, and if it can be proved that death from such causes is far from an extraordinary circumstance, we think the medical public have a right to demand that the proofs of poisoning be so clear that no reasonable man can have a doubt about the matter.

Yet how do we find that the case really stands? Of the medical witnesses examined upon the trial, ten were of opinion that the woman was poisoned, and seven that her death might be accounted for by natural causes. Here, at least, is a division of opinion in the skilled evidence, which should make us pause. Was it an irritant poison, or an irritable uterus, or the ulcerated bowel of dysentery, which did the poor lady to death? We confess that, as far as we can judge, her pregnant condition was quite sufficient to account for the symptoms under which she laboured.

There is one very remarkable circumstance which has not, we believe, been noticed: namely, that the commencement of the fatal illness of Isabella Bankes tallies very exactly with the commencement of her pregnancy. We have it in evidence that the vomiting and diarrhœa commenced about the 23rd of March, when Dr. Smethurst and the deceased were residing in Old Palace Gardens, Richmond; and that it continued until she died at Alma Villas on the 2nd of May—or about six weeks. We have it also in evidence that a fœtus of between five and seven weeks old was discovered by Mr. Barwell at the *post mortem* examination. This, to say the least of it, is a very singular coincidence, and one which should fairly tell in Dr. Smethurst's favour, inasmuch as he could not certainly be supposed to have so accurately timed the administration of deadly drugs from the day of her conception, which, of course, he could not have known.

But, it may be asked, how was it that the existence of pregnancy was never seriously inquired after, or even suspected? One would have thought that the symptoms were such that the possibility of the circumstance would have been immediately suspected by Drs. Julius and Bird, knowing, as they must have, that the parties were newly married. Yet we have no evidence whatever that any steps were taken to ascertain the fact by digital examination, which, in a woman of her age, labouring under a first pregnancy, would in all probability have cleared up the point, and possibly have suggested another cause than poison for the symptoms. Dr. Julius in his evidence dwells upon the fact, that no medicines appeared to him to have any effect in checking the distressing sickness and diarrhœa. Doubtless this circumstance must have appeared a very mysterious one, the fact of pregnancy not having been ascertained; but, viewed by this new light, the mystery does not seem so great. Dr. Herbert Barker, in a communication to this Journal, so late as the 30th of July last, gives a case which is pretty nearly a counterpart of that of the unfortunate Miss Bankes, with the exception that there was ultimate recovery. His patient, singularly enough, was of exactly the same age—forty-two; she became pregnant at the end of May 1857, and immediately after conception she was attacked with the vomiting of pregnancy, which continued with increasing violence for months, notwithstanding the administration of every known counteracting medicine. Early in August, diar-

rhœa supervened; and dysenteric stools, sometimes to the number of eight in a day, were passed. There were the same loathing of food and vomiting immediately on taking it; and the poor creature was reduced almost to death's door, when the sudden expulsion of a fœtus at once removed the more urgent symptoms, and opened out the path to recovery. In another case, similar to this, the patient died fourteen days after abortion, from the exhausting action of an intractable diarrhœa.

Dr. Tyler Smith, who speaks with authority in such cases, in his evidence stated not only that he had seen several cases in which death had resulted from the vomiting and purging of pregnancy, but that the burning sensation in the throat and the soreness of mouth in one case so exactly resembled the symptoms that would have resulted from the administration of poison, that he actually had the evacuations submitted to examination by a chemist. The terrified look, which Dr. Todd thought so diagnostic of poisoning, Dr. Smith had also seen in cases of the kind to which he referred; and Paul Dubois, the celebrated *accoucheur* of the French Empress, states that he has met with twenty instances of death from this cause in thirteen years.

It may be urged, however, that the mere simulation of symptoms of poisoning, which arise in some cases of pregnancy, afford no proof whatever that this was not a genuine case of poison. Here toxicology comes in to continue and complete the inquiry. In nearly every case of poisoning of late years, one of two things has been clearly proved; either poison has been found in unmistakeable quantities in the body, or it has been traced to the possession of the supposed murderer. Even in the case of Wooler, who was acquitted, arsenic was found in his possession. In the present case, however, no poison whatever was traced to the prisoner; no arsenic whatever was found in the viscera of the deceased, and but a very small proportion—less than a quarter of a grain—in four ounces of evacuated matter: a trace of antimony was also found in the large intestines.

In dealing with such minute quantities, if they are to be considered sufficient to weigh down the life of a human being, we ought to be very certain that no possible source of error could arise in the method of their detection. We are very far from having reduced Chemistry to an exact science. This trial, indeed, proves how far. Dr. Taylor swore before the magistrates at Richmond that he found arsenic in a bottle of solution of chlorate of potass in the possession of the prisoner; and it was inferred that the action of the last named drug upon the kidneys tended to eliminate the poison from the system; thus accounting for its absence from the tissues of the body. Here was an example of a false fact and a false inference, which must have told with terrible effect upon the prisoner. Had Smethurst been tried on this evidence directly after his committal by the Richmond magistrates, his life would have fallen through this illusive gauze wire; and Dr. Taylor might possibly, when too late, have discovered his lamentable error. This fact should, we think, act as a terrible warning to him; and we hope that he will not persist, as he said he should, in using the impure copper gauze in future experiments where human life is at stake.

If "traces" of irritant poisons, less than a quarter of a grain of arsenic (how much less?) in four ounces of evacuation are to be sufficient to hang a man, we feel that the public have a right to ask if there may not be many oxidising agents, even in the contents of the intestines, sufficient to set free the arsenic in impure copper? If so, and the man who holds in his hands the keys of life and death will not insist upon purity in his tests, then we say that the horrors which flourished in the days of witchcraft, when human life hung upon the lips of any old crone, will be but too faithfully represented by the horrors which will flow from the pseudo-scientific evidence of the present day.

But, strangely enough, Dr. Taylor, in his evidence before the magistrates, stated that he should attribute the symptoms exhibited by Miss Bankes to antimony rather than to arsenic; and certainly her symptoms did not tally with those seen in arsenical poisoning, even as described by himself. Possibly, then, antimony was the agent. But only a trace of antimony was found in the body, and none in the evacuations. Bismuth, however, which Mr. Rodgers asserts to contain antimony, was one of the drugs given to the deceased. How, then, shall we venture to say that the "trace" found was not attributable to the medicine given?

We confess that, in our opinion, the chemical evidence for the prosecution breaks down; and no manner of importance is to be attributed to it. Even if it were trustworthy, we do not think that such exceedingly nice scientific points as it involved could possibly be properly weighed by the "twelve good and true" but under-educated dull-brained Englishmen who compose an ordinary jury. We should not dream of weighing diamonds with a common steelyard: how much the more careful should we then be of allowing life and death to balance in the hands of twelve tradesmen, when the highest scientific attainments are scarcely equal to test the evidence in such a case!

The judge, indeed, seemed to think that it was of no possible consequence to the jury to dwell at all upon the scientific evidence. For instance, when the foreman of the jury fainted at the details of the *post mortem* examination, the judge said that "it appeared to him that it was quite unnecessary to go into these matters with such minuteness, particularly as the jury probably would understand very little of the matter." A more extraordinary statement than this never emanated from the judgment-seat. Why, the *post mortem* appearances of the intestines were of the utmost importance in enabling the jury to come to a proper conclusion, as to whether death arose from poisoning or from any other cause. It may be all very well for jurymen to faint under such an ordeal; but the judge should have at least remembered that the life of a human being was at stake, to whom these appearances were of the most vital importance. The conduct of the judge, however, in various parts of the trial, and especially in the summing up, was rather that of an advocate for the prosecution than of the occupant of the bench, holding fairly the trembling balance of justice.

Considering all these matters, we trust that execution will at least be stayed: and that, in accordance with the universally recognised principles of English justice, the unhappy prisoner will be allowed the benefit of those doubts which most men, who are competent to judge, feel with respect to the cause of the death of Miss Bankes.

Association Intelligence.

THE COLLEGE OF PHYSICIANS OF EDINBURGH.

The following resolution, proposed by Dr. Radclyffe Hall, and seconded by Mr. Wolstenholme, was adopted at the Annual Meeting of Council held in Liverpool:—

"That the recent proceeding of the Edinburgh College of Physicians, in the sale of their Licence without examination to gentlemen not already physicians, was not called for to meet any want in the profession; and that, inasmuch as it tends to lower the *status* of the physician, without elevating that of the general practitioner, it is calculated in every way to prove injurious to the body of the profession."

Philip H. Williams, M.D., *General Secretary.*

August 1859.

Reports of Societies.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, June 28th, 1859.

F. C. Skey, Esq., F.R.S., President, in the Chair.

DIFFERENT FORMS OF SYPHILITIC INOCULATION.

BY HENRY LEE, ESQ.

The object of this paper was to show that primary syphilis does not always commence in the same way. The "specific pustule," in which all syphilitic diseases were formerly said to originate, is produced by one kind of syphilitic inoculation only, and that form was one which does not give rise to constitutional or secondary symptoms. As nearly all the experiments on syphilisation had been performed so as to produce this pustular variety of the disease, it follows that no fresh constitutional syphilitic disease can be engendered by syphilisation so practised. The kind of syphilitic sore which infects the system commences in a different way, and when not artificially irritated, it gives rise rather to the adhesive than to the suppurative form of inflammation. This form of disease Mr. Lee had shown, in 1856, to be, as a rule, not inoculable upon the person who had it. This view had more recently been confirmed by the researches of French surgeons. But although not ordinarily inoculable like the suppurating form of the disease; yet it was capable of being rendered inoculable by artificial irritation. The results of the inoculation, were, however, then uncertain in their results, producing little local irritation, and capable of being transmitted by successive inoculations a very limited number of times. These observations applied only to inoculations performed upon the individuals who had at the time, or had previously had, infecting sores. The author described one kind of suppurating sore which was surrounded by induration, which could not always be distinguished from the induration of the infecting sore. The induration could not, therefore, always be taken as the diagnostic mark of a sore which would infect the patient's system. The character of the secretion, however, gave the information which the induration did not always give. If care were taken to prevent any accidental cause of irritation, the secretion from an infecting sore would soon cease to be purulent, whereas, in the suppurating sore surrounded by induration (the phlegmonoid variety of suppurating sore) the secretion would continue, as in other forms of suppurating sores, puriform to the last. The number of cases of indurated sores which had been said to have been inoculated by Dr. Sperino and others, led to the conclusion that the two forms of disease now described had not been distinguished from each other. It was now ascertained that the infecting sore could not, as a rule, be inoculated upon the patient having it, whereas the phlegmonoid variety of the suppurating sore was of all kinds the most readily inoculated. When inoculated artificially, it produced a pustule containing well formed pus within forty-eight hours, and it was occasionally followed by an eruption of brick-red colour, confined to one part of the body, disappearing spontaneously, and not recurring. This eruption was, therefore, certainly not syphilitic.

ON THE ADMINISTRATION OF BELLADONNA, AND ON CERTAIN CAUSES WHICH MODIFY ITS ACTION.

BY HENRY WM. FULLER, M.D.CANTAB., F.R.C.P.L.

Dr. Fuller was led to the inquiries which form the subject of this paper, by observing the remarkable tolerance of belladonna exhibited by a child, a patient in St. George's Hospital, to whom he was administering it as a remedy for chorea. Fancying that the tolerance of the drug observed in the case in question might be attributable either to imperfection of the extract, or to the modifying influence of the choreic spasms, he obtained other extract of belladonna from Apothecaries' Hall, from Squire's, and from Jacob Bell's, in Oxford Street, and administered it, dissolved in water, to ten other choreic patients in the hospital. In a twelfth case he administered atropine, obtained from Morson's, in Southampton Row. The result was in all cases the same; namely, extraordinary tolerance of the remedy, with a varying, but not very satisfactory, effect as regards the subjugation of the choreic spasm—the tolerance of the drug being so great that one girl, aged ten, took seventy grains of the extract of belladonna daily, and a total amount of 1,019 grains, or rather more than two ounces, in twenty-six days; whilst the child, aged fourteen, to whom the atropine was administered, took no less than thirty-seven grains in eighteen days.

1. The patients were all pale whilst taking the larger doses of the drug; and in no instance was there any feverish heat, or any rash, or erythematous blush on the skin. 2. There was great weakness of the pulse in all the cases, and in some, considerable quickness. 3. The urine was generally clear and acid, but scanty, and of high specific gravity, varying from 1,024 to 1,036. In three cases it frequently contained a copious deposit of crystallized lithic acid; and, in three other cases, it was usually loaded with lithates. In one case, for the space of a few hours, whilst the patient was under the toxical influence of the drug, it became ammoniacal almost as soon as voided. 4. In one case some difficulty was experienced in voiding the urine; but this was not observed in any other case. This difficulty passed off when the belladonna was omitted. 5. The tongue was always moist, but unusually red whilst the larger doses of belladonna were being taken, and the redness passed off when the drug was omitted. 6. The remedy did not, in any instance, exert a constipating effect; on the contrary, it appeared to prove aperient. An occasional purge was required in three cases only. 7. In five cases it ultimately gave rise to sickness and diarrhœa; but in every instance save one, the choreic spasms had almost wholly ceased, and in the exceptional case alluded to, had greatly subsided before those symptoms were produced. Whenever bowel symptoms occurred, mere omission of the medicine sufficed to cause their cessation. Did the existence of spasm counteract the influence of the drug, and prevent their occurrence? 8. Dilatation of the pupils was very uncertain. In almost every instance the pupils were large before the administration of the medicine was commenced, and they invariably became dilated soon after a dose of the medicine was taken. The dilatation, however, was not to the degree observed when a solution of belladonna is dropped into the eye, and, in most of the cases, it passed off before another dose of the medicine was due. Its ordinary duration was about two hours and a half. In one case, excessive dilatation occurred for a few hours coincidently with the occurrence of sickness and purging. In two cases, considerable dilation was pretty constant; in one case it was seldom great. 9. In two instances only did the slightest indistinctness of vision occur. In one of these it was observed only on three occasions, and then only to a slight degree, and was not accompanied by dryness of the throat, headache, or any impairment of the mental faculties; in the other it took place more frequently, and strange to say, was most complained of when the pupils were of their natural size, and were contracting freely under the stimulus of light. It was not attended by delirium, nor by any indication of the action of belladonna, and the administration of an additional quantity of the drug was almost invariably followed by its removal. 10. The drug did not in any case produce the slightest narcotic effect; and in one case it failed utterly as an anodyne. 11. In no instance was there any evidence of its accumulation in the system. 12. The tolerance of the drug was not in proportion to the severity of the choreic spasms. In Case 2, in which fourteen grains of the extract, daily, occasioned sickness and purging; the spasms were more severe than in Case 11, in which seventy grains were taken daily without disturbance of the stomach and bowels. 13. The curative effect of the drug was very uncertain. In seven cases its action ap-

peared to be decidedly curative, but in two cases it failed to exercise the slightest control over the spasms; and in the other three cases it is doubtful whether the improvement ought to be attributed to its action.

Being desirous of ascertaining whether the tolerance of the drug was due to its decomposition in the stomach, or to its non-absorption, the author submitted to Dr. Marcet and Mr. Kesteven, for examination, some of the urine voided by a patient who at the time was taking sixty-four grains of the extract of belladonna daily. The former extracted atropine enough from three ounces of the urine to kill two white mice, and narcotise several others. The latter, from two ounces of the urine, obtained sufficient to produce dilatation of a cat's eye, to afford filamentous crystals of atropine, and to give the reactions which atropine yields with iodine water, tannic acid, chloride of gold, and sulphuric acid, and bichromate of potash. The fæces also, on being analyzed by Dr. Marcet, yielded abundance of atropine.

Thus, then, up to this point, five facts appeared proved; viz.: 1. In cases of chorea extraordinary large doses of belladonna and atropine are tolerated. 2. The drug is absorbed into the blood, and therefore that the tolerance of it is not attributable to its non-absorption, nor to its being decomposed in the stomach. 3. It does not accumulate in the blood, but passes out of the system with the urine and fæces, and probably with the other excretions. 4. It does not exercise that amount of control over the choreic spasms which would have been expected from the readiness with which it is tolerated by the system. 5. The tolerance of the remedy is not in proportion to the severity of the choreic symptoms.

The question therefore arose as to whether the existence of chorea had any part in producing tolerance of the drug, or whether that tolerance may not have been due to some other circumstances? With the view of determining this point, the author administered the extract of belladonna to two convalescent children, whom he kept in the hospital for the purpose. To the one, aged seven, he ultimately gave thirteen grains of the extract daily; and to the other, aged ten, twenty-eight grains daily; without producing dryness of the tongue or fauces, or any symptom indicative of the action of belladonna beyond some temporary dilatation of the pupils.

With the view of having the matter tested with children on a larger scale than is possible at St. George's Hospital, the author requested a friend who is attached to a large public institution for children, to administer it cautiously, in gradually increasing doses. Accordingly, to eleven children, varying in age from three to six, one-eighth of a grain of the extract, in solution, was administered three times a day, and the dose was increased, in the course of six days, to half a grain thrice daily. These children were all in good health. The dose was gradually increased, and dilatation of the pupil was the only effect produced. To seven other children, between five and seven years of age, he began by giving one-third of a grain twice a day, and continued it for three days without perceiving any effect from its administration beyond slight dilatation of the pupil. He then prescribed two-thirds of a grain, twice a day; but, by mistake, one grain and a third was given at a dose. The result of this large and sudden increase was that the children were all seized with sickness and vomiting; some of them had diarrhœa, and one of them had the violent uncontrollable delirium characteristic of belladonna. Stimulants were at once administered, the belladonna was omitted, and on the following day the toxical effects of the drug had passed off, and the children were perfectly well. To adults the author administered the drug in pills and in solution, and he found that, however given, very small doses usually produce dryness of the tongue and fauces; that two grains daily will often excite vertigo and dizziness, and that it is not possible to establish a tolerance of the larger doses, as in children.

He was thus led to the conclusions that:—1. The tolerance of belladonna is not attributable to the counteracting influence of choreic spasms, but is in some way connected with the age of the patient. 2. A much larger dose than is usually prescribed is well borne from the first by children of tender years. 3. In children, though not so in adults, a tolerance of the remedy is speedily established, so that the dose may be safely increased, rapidly, but gradually. 4. Special care should be taken in apportioning the dose to the age of the patient, and in not increasing the dose too rapidly, inasmuch as the usual toxical effects of the drug will be produced if too large a dose be given before a sufficient tolerance of the drug has been established. 5. The milder toxical effects produced by the drug are of little importance, and subside without remedies as soon as the administration of the medicine is discontinued. 6. Adults cannot tolerate the doses of the drug which can be taken with impunity by children.

The extraordinary difference in the tolerance of the drug observed at different periods of life might be explicable by the medicine passing off with the urine, as also, probably, with the other excretions, more rapidly in childhood than in adult life. The author concluded his paper by the following suggestions:—1. Inasmuch as belladonna is admitted to be productive of signal benefit in whooping cough, even in the minute doses in which it has been hitherto administered, it is probable that a corresponding increase of benefit would result from the larger doses, which it is now proved may be safely prescribed under certain restrictions. 2. It deserves a trial in epilepsy, laryngismus stridulus, and other spasmodic affections. 3. Combining as it does antispasmodic, sedative, and slightly purgative properties, it may be productive of relief in certain cases of dyspepsia connected with inframammary pain, flatus, and spasms in the abdomen. 4. Inasmuch as it exercises a remarkable power in controlling spermatorrhœa and incontinence of urine, and the experiments recorded in this paper prove that it is excreted with the urine, it is highly probable that its curative action in such cases may be due in great measure to its topical effect, and if so, that it might be applied locally with advantage.

EXTENSIVE ADHESION OF THE INFERIOR MARGIN OF THE SOFT PALATE TO THE POSTERIOR WALL OF THE FAUCES.

BY WM. TURNER, M.B.LOND.

[*Communicated by* James Paget, Esq., F.R.S.]

The author described a curious case of palato-pharyngeal adhesion, which came under his notice in the dissecting-rooms of the University of Edinburgh. There was a complete absence of the uvula, and there was no communication between the back of the mouth and the nasal part of the pharynx, except through a small rounded hole situated immediately on the left side of the middle line, and corresponding apparently with the upper part of the arched orifice usually found at the base of the uvula. This separation of the nasal from the buccal parts of the pharynx was due to the adhesion of the inferior margin of the soft palate to the interior of the pharyngeal wall, apparently along the folds of mucous membrane containing the palato-pharyngei muscles. The mucous membrane, especially about the line of junction, presented a cicatrised appearance. The pharynx was very much contracted laterally, being not more than half an inch across immediately opposite the hamular processes; this contraction corresponded exactly with the line of union of the soft palate to the interior of the pharyngeal wall. The upper part of the pharynx consequently presented a funnel shape. The greater part of the fibres of the superior constrictor muscles had their place occupied by a thickened fibrous membrane; the right stylo-pharyngeus muscle was also greatly altered at its lower part by fibrous thickening. The other pharyngeal muscles, and those portions of the tensores and levatores palati which were situated outside the pharynx, presented their normal appearance. By removing the roof of the nasal part of the pharynx, the interior of this portion of the tube was observed to be shaped like a funnel or inverted cone, the floor being formed of the obliquely inclined soft palate; and at the most depending part, the small rounded aperture of communication with the buccal part of the pharynx was seen. The mucous membrane on the upper surface of the soft palate exhibited the same roughened appearance as on the under. No history of the case could be obtained; but the author inferred that, from the cicatrised appearance of the pharyngeal and palatal mucous membrane, the thickened and roughened condition of the posterior pharyngeal wall, the replacement of muscular fibres by fibrous tissue, and the constriction of the part, that the change from the normal state must have been produced by severe inflammation at some former period.

The author then referred to the only two recorded cases of a similar nature he has been able to meet with—one by Rudtorffer, in his *Abhandlung;* the other by Otto, in his *Handbuch der Pathologischen Anatomie.*

SEQUEL OF A CASE OF A CALCULUS IN THE BLADDER REMOVED BY LITHOTRITY: COMMUNICATION BETWEEN THE BLADDER AND INTESTINE. BY CHARLES HAWKINS, ESQ.

The patient, whose case is related in the last volume of the *Medico-Chirurgical Transactions* (see also British Medical Journal, August 26th, 1858, p. 734), died on April 19th, 1859, a year after the operation. On a *post mortem* examination,

no stone was discovered in the bladder. The kidneys were somewhat congested, but in other respects healthy in appearance. There was an opening in the bladder at the lower part of the posterior wall, of the diameter of a goose-quill, evidently not of recent date. The bladder, corresponding to this aperture, was intimately united by old adhesions to that part of the circumference of the sigmoid flexure of the colon that lies nearest it. The aperture in the bladder communicated with the sigmoid flexure opposite their point of union. Above the point of communication of these two viscera, for the extent of about an inch, the canal of the sigmoid flexure was somewhat constricted; but this constriction was apparently due to the adhesion and subsequent contraction of these viscera, as beyond the point where adhesion between them existed, the calibre of the sigmoid flexure appeared normal. Below the orifice of communication between the bladder and colon, the canal of the intestine was greatly constricted to the extent of an inch and a half in length; this stricture appeared to depend upon great condensation and subsequent cicatrisation of the submucous and muscular tissues of the bowel at that point. The mucous membrane of the intestine above the seat of stricture presented in many places pouches varying in size from that of a pea to that of a filbert, and formed by the protrusion of this coat externally. Opposite to the stricture it appeared to be in every respect quite healthy, but very densely convoluted. Below the seat of stricture the bowel was considerably dilated, and had during life apparently acted the part of a second bladder, as, from the symptoms described by the patient, the urine used to accumulate there in considerable quantities, being passed *per anum*.

Editor's Letter Box.

SUSPECTED ARSENICAL POISONING: FALLACY OF THE COPPER-TEST.

LETTER FROM J. C. S. JENNINGS, ESQ.

SIR,—About two years since (July 1857), I was in attendance on a married woman whose symptoms so closely resembled those of slow arsenical poisoning, that I at once administered an antidote, after having tested the evacuations, and obtained, as I conceived, arsenic by the copper gauze test. The fæces were peculiarly sloughy and offensive, quite unlike those of diarrhœa or dysentery; and medicines failed to relieve the patient. Moreover, on communicating my suspicions to the wife, I found that she had frequently suspected her husband of intending to poison her, and remembered being very sick after having taken ale administered by him. Besides this, I had the opinion of an intelligent physician, who visited her with me, and thought the symptoms very suspicious, and not to be accounted for in any other way; there was the remarkable anxiety of countenance, and pinched expression attributed to such cases; and, moreover, during the following night and day spent in analysis, I had the assistance of an experimental chemist.

I subsequently sent the evacuations, fæces and urine, napkins, etc., together with herb-tea given her by her husband, to Mr. Herapath, of Bristol, who declared that he could find no trace of mercury, antimony, or arsenic. The case shortly afterwards was placed by the husband under the care of another medical man, and soon terminated fatally. The husband took possession of the furniture, and even wearing apparel of his mother-in-law, who resided with them during his wife's lifetime, and turned her out of doors.

Here, then, was a motive for bringing about the death of his wife, whom he had frequently treated harshly during life; and yet I doubt not that the arsenic obtained by me was contained in the copper used as a test, and many reasons lead to this conclusion. I am, etc., J. C. S. JENNINGS.

Abbey House, Malmesbury, August 24th, 1859.

OPERATION FOR STONE IN THE FEMALE.

LETTER FROM W. MCEWEN, M.D.

SIR,—In answer to "Embryo's" note, inserted in your last JOURNAL, regarding the operation for stone in the female, I have to observe that the urethra and neck of the bladder were cut through; the knife was then carried with the edge directed to the ramus of the os pubis for a little way, then obliquely outwards and downwards. Perhaps the word *laterally* might be apt to mislead; but it was used in reference to the position of the patient. I am, etc.,

W. MCEWEN.

27, Nicholas Street, Chester, August 24th, 1859.

Medical News.

BIRTHS, MARRIAGES, DEATHS, AND APPOINTMENTS.

** In these lists, an asterisk is prefixed to the names of Members of the Association.*

BIRTHS.

Of sons, the wives of—

*BERNEY, Edward, Esq., Croydon, on August 23.
*CHEVALLIER, B., M.D., Ipswich, on August 12. (Stillborn.)
CORNER, Matthew, M.D., Mile End Road, on August 16.
*PEMBERTON, Oliver, Esq., Birmingham, on August 18.

Of daughters, the wives of—

FULLER, Henry W., M.D., Manchester Square, on August 11.
*TUKE, Harrington, M.D., Chiswick, on August 19.

MARRIAGES.

DANIELL, Wm. C., Esq., Surgeon, of Stoney Stratford, son of *E. Daniell, Esq., of Newport Pagnell, to Sarah Bland, daughter of G. BATTAMS, Esq., of Curlton, Bedfordshire, on August 18.

DUNCAN, Thomas, M.D., of Richmond, to Fanny E., only daughter of the late A. MORPETH, Esq., of Bishop Wearmouth, on August 23.

HOME, William, M.D., Surgeon-Major, to Charlotte, daughter of F. GROME, Esq., Mount Tallant, Dublin, on August 17.

HUDSON, John, Esq., Assistant-Surgeon R.N., to Marian Sinclair, only daughter of Alexander WATSON, Esq., late 1st Dragoon Guards, lately.

KNIGHT, Wm., Esq., 90th Light Infantry, to Cornelia Mary, elder daughter of *George BURY, Esq., of Whetstone, on August 17.

MOBERLY, Wm. H., Esq., Southampton, to Mary Arnoth, eldest daughter of the late Robert KNOX, Esq., Surgeon R.N., on August 20.

MORGAN, Allen B., Esq., Surgeon, of Wagga Wagga, New South Wales, to Emma Louisa, third daughter of Francis R. HUME, Esq., Castlesteads, Australia, on May 4th.

DEATHS.

BENNETT, F. D., Esq., Old Kent Road, aged 51, on Aug. 22.

BURNS. On August 20th, John R., son of *J. J. D. Burns, M.D. R.N., Brompton, Chatham, aged 11 months.

TILSLEY, H. N., Esq., North Petherton, aged 60, on Aug. 21.

WILKINSON. On August 16th, at Manchester, aged 2¼, Roger A. E., only son of *M. A. Eason Wilkinson, M.D.

YELL, William H., M.D., aged 54, on August 18.

HEALTH OF LONDON—AUGUST 20TH, 1859.

[From the Registrar-General's Report.]

		Births.	Deaths.
During week	Boys .. 917 / Girls .. 864	1781 ..	1188
Average of corresponding weeks 1849-58..		1555 ..	1185

Among the causes of death in the week were—diarrhœa, 240; cholera, 10 (8 children and 2 men); small-pox, 20 (including 14 children under 5); measles, 14; scarlatina, 51; diphtheria, 17; whooping-cough, 23; fever, 37; consumption, 140; accident or negligence, 28; murder, 1; suicide, 1; sudden deaths without stated cause, 3.

Barometer:
Highest (Sat.) 30.021; lowest (Mon.) 29.66; mean 29.902 in.

Thermometer:
In sun—highest (Sat.) 106°; lowest (Wed.) 72°.
In shade—highest (Sat.) 85°; lowest (Tu.) 47.5°.
Mean—63.9°; difference from mean of 43 yrs. +2.7°.
Range—during week, 37.5°; mean daily, 21°.
Mean humidity of air (*saturation*=100), 77.
Mean direction of wind, variable.—Rain in inches, 0.22.

TRIAL OF THOMAS SMETHURST.

The trial of Thomas Smethurst for the murder of Isabella Bankes, after having been interrupted at the July sessions of the Central Criminal Court by the sudden illness of a juror, was recommenced at the same court on Monday, August 15th, and terminated on the following Friday. The prisoner pleaded "Not Guilty" to the indictment charging him with murder. The Lord Chief Baron Pollock presided at the trial. Mr. Sergeant Ballantine (specially retained), Mr. Bodkin, Mr. Clark, and Mr. Merewether were counsel for the Crown; Mr. Sergeant Parry was specially retained, with Mr. Giffard, to defend the prisoner.

On the part of the prosecution, the following facts were alleged:—The prisoner was a member of the medical profession. He had been married in 1828 to a wife about 28 years older than himself, and who was now living, at the age of 74. The prisoner first comes under notice as lodging with his wife at No. 4, Rifle Terrace, Bayswater. While they were living there, Miss Isabella Bankes also came there to reside. She was a lady, aged 42, of respectable family, and was possessed of property under her own control to the extent of £1,700 or £1,800; and in June 1858, she became possessed of a life interest in £5,000, which at her death went to the other members of her family. In the autumn of 1858, after her father's death, she appeared to have been living at this lodging-house, although she had relatives living in the neighbourhood. An intimacy soon sprang up between her and Dr. Smethurst and his wife. In November, the landlady, considering the intimacy between Dr. Smethurst and Miss Bankes too great, spoke to her about it, and that lady left the house on November 29th. The prisoner also quitted on December 12th, leaving his wife, whom he visited once or twice afterwards. The landlady of the lodging house, Mrs. Mary Smith, stated that the deceased appeared to be in very delicate health. On two occasions she was very sick, and she repeatedly said that when she attempted to take food she had nausea. On one or two occasions she was obliged to leave the dinner table, and appeared unable to take her regular food. After leaving this place the deceased lodged at a house in Kildare Terrace, Bayswater, from the 29th of November to the 9th of December. During this interval she went out to walk every day. When the deceased left this house she said she was going to Clifton for change of air. On December 9th (or 12th?) Smethurst and Miss Bankes were married at the parish church of Battersea. Nothing more seems to have been heard of them until January 28th, when Miss Louisa Bankes, the sister of the deceased, received a letter from her. On February 4th, the prisoner and the deceased, under the name of Mr. and Mrs. Smethurst, took apartments in the house of Mrs. Anne Robinson, Old Palace Gardens, Richmond, and remained there until April, when they left in consequence of an increase of rent being demanded. This witness stated that the deceased seemed to be in good health when she first came to the house; but about three weeks before she left she suffered from diarrhœa and sickness. The prisoner said he should call in a medical man, and witness advised him to employ Dr. Julius, of Richmond, who attended upon the deceased until she left. The vomiting and sickness continued all the time the deceased remained in the witness's house. The prisoner supplied the deceased with all her food. Witness used to hear the deceased retching early in the morning. As soon as she began to move it appeared to bring on the sickness. The deceased frequently complained of want of appetite. The daughter of the last witness confirmed her testimony, and said that once she gave the deceased an egg, and she ate it, and said she enjoyed it. This witness never noticed that the deceased vomited without taking anything; and the vomiting happened while the prisoner was absent as well as when he was with her. The prisoner and deceased, on April 15th, went to lodge at the house of Mrs. Susannah Wheatley, in Alma Villas, Richmond. The deceased appeared very ill, and went to bed immediately. The prisoner always took her food, and he used to place the bedroom utensils on the landing, and witness emptied them. The witness said she once suggested that she should act as nurse to the deceased, and sit up with her; but Smethurst said that he could not afford it, and that no one would wait upon her so well as he could. The bedroom of the deceased was never thoroughly cleaned. There was a secretary on the landing which had two keys, and the prisoner kept them both. The prisoner told witness to keep one evacuation for Dr. Julius to see it. Witness once made some tapioca for the deceased, and the prisoner told her afterwards that the deceased said it was very nice. She made some another time, and the prisoner said that Mrs. Smethurst told him it was bitter.

The evidence of Miss Louisa Bankes, sister of the deceased, was to the effect that she and the deceased had formerly lived together; but last year the deceased went to live at Mrs. Smith's, and witness visited her there. She saw nothing of her after she left Kildare Terrace until April following. On the 20th of that month, she received a letter from the prisoner, requesting her to see her sister at No. 10, Alma Villas, Richmond. The deceased appeared rather agitated when she saw her, and said that if witness would be quiet it would be all right. Witness remained with the deceased for several hours; but the prisoner was present all the time except for a minute or two. Witness suggested that a medical gentleman, named Lane, a relation of the family, should see the deceased; and she said she would rather not see him, and the prisoner said he thought it would be better not. There was some tapioca in the room, and her sister said it had a nasty taste, and witness proposed to make some; but the prisoner said the milk had not come. She then said she would make the deceased some blanc-mange, but the prisoner objected. The prisoner shortly after this gave the deceased a saline draught, and she was very sick immediately. When witness left, the deceased expressed herself very anxious to see her again, and she promised to come again in the next week. The prisoner afterwards wrote to her, requesting her not to come, as her sister could not bear the excitement of seeing her, and the doctors had forbidden it. In consequence of another letter she received from the prisoner on the 30th of April, she went again to Richmond, and saw the prisoner. He did not say anything to her about her sister having made a will that morning. Upon going into her room the deceased recognised her, and held out her hand; but she was unable to speak. The prisoner was present all the time, and he asked her to go downstairs; and she went down with him, and he left her downstairs and went up to the deceased's bedroom. She had brought some soup with her in a jelly, and the prisoner put some warm water to a portion of it, and then took it out of the room, for the purpose, as he said, of cooling it; and she heard him stirring it. Very soon afterwards he gave the soup to the deceased, and she brought it up immediately. The prisoner then told her that her sister was so ill she had better not remain, and he also told her that some pills Dr. Todd had prescribed for her had given her great pain, and had caused her burning sensations all over the body. On the Monday (May 2nd) after this occurred, the prisoner was taken into custody; but he returned in two or three hours very much excited, and he said that Dr. Julius had charged him with poisoning her sister, and that they (Dr. Julius and the other doctors) were killing her. Witness and a nurse attended upon the deceased after this until her death, and they gave her arrowroot and other things of that description, and she did not vomit on any one occasion. She also asked for some tea, and that was also retained on her stomach. The deceased died on the 3rd of May, at eleven o'clock. After the prisoner returned, witness asked him to give out something for the nurse; but he refused, and said that witness had taken the responsibility upon herself. The health of the deceased was generally very good. The deceased used to suffer occasionally from bilious attacks and sickness, and if she rode in a carriage it made her vomit. The deceased always represented that the prisoner was very kind to her. A nurse who had been engaged to attend on the deceased on May 2nd (the day preceding her death), stated that on the first two occasions when she gave medicine to the deceased it was vomited; but that food and medicine afterwards given was retained. At the request of the prisoner she placed some of the vomited matters in a jar, which was sealed by the prisoner and given to a policeman. Mr. Senior, a solicitor of Richmond, said that on the 30th of April he made a will, by which the deceased left the whole of her property, with the exception of a brooch, to the prisoner, whom she described as "her sincere and beloved friend." The prisoner was appointed sole executor of the will. The document, with the exception of the signatures, was in the handwriting of the prisoner. The deceased was described in the will as a spinster. Mr. W. Tarte proved that the deceased lady was possessed of a sum of £1,740, which was lent out on mortgage. She also had a life interest in the sum of £5,000. On May 2nd the prisoner wrote a letter to his wife, stating that he had not been able to leave Richmond in consequence of his professional aid being required; but that

he would see her as soon as possible. If anything should prevent his coming to town he would send her some money.

The professional evidence for the prosecution commenced with the examination of Dr. Julius, who attended upon the deceased lady from the 3rd of April until her death. The prisoner told him that she had been suffering from diarrhœa and vomiting for a week. Dr. Julius believed it was a case of simple diarrhœa, and prescribed some chalk medicine. The following morning he was shown a vomit of a grassy green colour. He saw the deceased again on the 5th of April, when she appeared to be much worse. The sickness was very constant and distressing, and the prisoner complained that she could not take any food. The prisoner proposed that a mixture containing prussic acid should be administered. Witness did not think it would do any good, but upon the prisoner saying that he would watch its effects the mixture was made up and given; but it did not appear to have any effect. The sickness and retching continued unabated, and the deceased complained of burning pains in the mouth and throat, and all down the stomach; and said that even the act of swallowing caused her to vomit. There was no appearance of bile in the evacuations of the deceased after the fourth or fifth day that he saw her. On the 8th of April more serious symptoms exhibited themselves, and the deceased appeared to be fast sinking. The same symptoms continued after taking every one of the medicines given. The witness said that, in consequence of the suspicions aroused in his mind, he requested his partner, Dr. Bird, to see her, but he did not give him any hint of his suspicions. Witness was satisfied that some irritant was being administered which counteracted the effects of the medicine and kept up the symptoms. Dr. Bird attended upon the deceased from the 18th of April to the 21st, when witness saw her again, and she then appeared very much worse. The prisoner suggested that other medical advice should be called in, and on the 28th of April Dr. Todd saw the deceased. Witness did not communicate his suspicions to Dr. Todd. Dr. Todd prescribed a pill of sulphate of copper and opium. Witness had previously recommended the same description of pill; but the prisoner objected to sulphate of copper. The witness procured one of the evacuations of the deceased: at this time the pills prescribed by Dr. Todd had not been taken by her. In consequence of what he afterwards heard from Dr. Taylor, the witness applied to a magistrate, and the prisoner was taken into custody on Monday, the 2nd of May. The witness said he was not aware of the deceased having been visited by her sister until the 30th of April, and that such a subject was never alluded to, and he was never told that the deceased had made a will. Dr. Julius believed that small doses of an irritant poison would have produced all the symptoms, and said that no such ingredients were contained in any of the medicines sent for her by him. On the prisoner's application, after he had been taken into custody upon the present charge, he gave him full information of everything that had been prescribed. The prisoner always appeared to make a frank statement of the symptoms, and he described them correctly according to witness's observations. To the best of his recollection he was only twice alone with the deceased; but he would not swear that he was not alone with her oftener. The prisoner first suggested that further medical assistance should be obtained. The medicines witness administered to the deceased were chalk mixture and catechu, compound tincture of camphor, grey powder, and pills of compound powder of ipecacuanha; also quinine, diluted sulphuric acid, diluted prussic acid, ether, and gentian. He never prescribed bismuth or nitrate of silver, or acetate of lead. Until the *post mortem* examination he had no means of ascertaining that the deceased was pregnant, and it would have made no difference in her treatment if he had been aware of it. Vomiting was a very common symptom of pregnancy, and it was sometimes accompanied by diarrhœa. Witness had never heard of a pregnant woman suffering from severe vomiting and diarrhœa which would not yield to any ordinary remedies. He had never made the *post mortem* examination of a body that had died from arsenic or antimony. On the 8th of April the deceased could not have been pregnant longer than a few days, and at that early period witness never heard of pregnancy being accompanied by diarrhœa. The symptoms were totally different from those generally exhibited by pregnant women. The burning sensation in the throat and soreness of the mouth were quite unusual in the vomiting of pregnancy.

Dr. S. D. Bird, the partner of Dr. Julius, visited the deceased lady on or about the 18th April. Dr. Julius did not tell him before he went that he entertained any suspicion in reference to the illness. The prisoner told him he thought the deceased had neglected her bowels, and that they were unloading themselves by diarrhœa and vomiting. Witness continued Dr. Julius's prescription for two or three days, and he then changed the treatment. The symptoms went on increasing in violence. Bismuth, acetate of lead and opium, and nitrate of silver, were afterwards administered; and the prisoner said that the nitrate of silver had occasioned a burning pain all through the bowels. After witness had been in attendance for three days, he formed an opinion that something was being administered which had the effect of counteracting the medicine, and he communicated this opinion to Dr. Julius. On the 30th of April he asked for a portion of one of the evacuations of the deceased, and the prisoner gave him about four ounces, which was placed in a jar and sealed, and he then gave it to his assistant, Mr. Caudle. Another evacuation was afterwards procured, and they were numbered one and two, and were both sent to Mr. Buzzard, of Great Marlborough Street, to be examined. On Monday, May 2nd, prisoner showed witness an evacuation, a portion of which witness took away with him, and it was sealed, labelled, and numbered three. The prisoner was taken into custody on that day, and was admitted to bail on his own recognisance. M'Intyre, the officer, accompanied witness to the prisoner's residence, and he handed him some bottles and pill-boxes that he found there. Witness on this day several times gave the deceased arrowroot and brandy, and beef-tea and brandy, and they always remained on the stomach. On the following morning, witness was sent for suddenly to the house, and found the deceased evidently dying; but there was no vomiting or diarrhœa. Witness believed that death was occasioned by the irritation produced by the constant administration, in small doses, of some mineral poison: arsenic or antimony would be calculated to produce all the symptoms exhibited by the deceased. Antimony would have been a most improper thing to administer to the deceased; and witness never administered any in the medicines he prescribed. The symptoms were not attributable to any known natural disease: he had seen much dysentery in the Crimea, but never saw such a case as this. He had never been present at a *post mortem* examination in slow poisoning by arsenic. Corrosive sublimate would have produced the same results and symptoms as appeared in this case: salivation would not necessarily be produced by it.

Mr. Caudle, the assistant of Dr. Julius, gave evidence as to the fact of the medicine prescribed having been made up by him; none of them contained arsenic or antimony. The son of Dr. Julius stated that he had, on April 30th, taken two sealed bottles, given him by Dr. Bird, to Mr. Buzzard, in Great Marlborough Street, and Mr. Buzzard gave evidence of having received the bottles and given them to Dr. Taylor, with a letter from Dr. Bird.

Mr. Richard Barwell on the 4th of May made a *post mortem* examination of the deceased, assisted by Mr. Palmer. Dr. Julius, Dr. Bird, and Mr. Bird were present, but they took no part in the examination. The arms were flexible, but the legs were very rigid. The face was of a dull earthy colour, and very emaciated, and the lips were drawn in. The brain was perfectly healthy, and so were the lungs; the liver was hard and of full size. (It was stated by Mr. Palmer to be enlarged and hardened, and in an incipient state of fatty degeneration.) The deceased appeared to be between five and seven weeks advanced in pregnancy. The heart was healthy. Upon examining the stomach externally, he found at one end a large black patch, but there were no ulcers nor any appearance of acute inflammation. He had never made a *post mortem* examination in any case where death was the result of slow poisoning.

Dr. Wilkes said that, in his opinion, the death of the deceased was most probably to be accounted for by some irritant. Excluding dysentery, he was not acquainted with any form of disease that would present similar symptoms.

Dr. Todd, physician to King's College Hospital, said that when he saw the deceased, he noticed a peculiar expression of countenance,—an expression of terror. The abdomen was very hard; and he was at once under the impression that she was suffering from the influence of some irritant poison. By witness's desire an evacuation was obtained, and he directed Dr. Julius to make up the sulphate of copper and opium pills. Witness had never known any bad effect produced by this medicine, and he did not think it possible it could produce a burning sensation in the throat or the stomach. If the disease had been diarrhœa and bowel complaint, the medicines administered by Dr. Julius were proper. He had never been present at the *post mortem* examination of a body where death arose from slow irritant poisoning. Vomiting and diarrhœa

might occur in early pregnancy, but he did not believe that in any stage of pregnancy there could be such an extensive ulceration of the intestines as existed in this case. The only disease that could in any way account for the symptoms of the deceased would be what is called acute dysentery; but he believed her death was caused by the administration of an irritant poison.

Dr. Metcalfe Babington, Dr. L. Bowerbank, and Dr. Copland, stated that they had seen many cases of acute dysentery, and that the symptoms were not reconcileable with that form of disease. They believed death to have been the result of the administration of an irritant poison.

Dr. A. S. Taylor said that, on Sunday, the 1st of May, Mr. Buzzard brought him two bottles, containing matters to be examined. From an examination of one of these, containing about four ounces of an evacuation, he became satisfied of the presence of arsenic by the copper test. His calculation was that there must have been less than a quarter of a grain mixed with the four ounces of matter in the bottle. There was no trace of mercury, or antimony, or bismuth, but he discovered the presence of copper by a subsequent test. He recommended that hydrate of magnesia should be administered to the patient. He afterwards examined the contents of the other bottle, and found that it did not contain any poison or any metallic matter whatever. On the 5th and 7th of May, Inspector M'Intyre brought him a portion of human viscera: also a number of bottles, and several pill-boxes. There were altogether twenty-eight articles; he examined the whole of them for arsenic and other poisons, but did not discover any. Dr. Taylor said that he discovered antimony in the lower intestines, and also in the cæcum, and upon one of the kidneys. There were also traces of antimony in some blood that was said to have been taken from the heart. Dr. Odling and himself came to the conclusion that the quantity of antimony present was about half a grain. The intestines presented appearances of excessive inflammation, and the lining membrane was almost entirely destroyed. There was likewise blood in the stomach. The medicines that had been supplied by Dr. Julius contained the ingredients of which they were represented to be composed. In one of the bottles, found in possession of the prisoner, there were 355 grains of chlorate of potass. Another bottle appeared to contain a clear saline liquid. He tested a portion of the contents by Reinsch's test, and, upon first trying the copper, it was entirely consumed. He made a further test, which led him to suppose that there was arsenic present in the solution, but it turned out that it did not contain either arsenic or antimony, and that the arsenical appearances came from the copper gauze. By the destruction of the copper the arsenic contained in it was set free. The chlorate of potass destroyed the copper. Dr. Odling also came to the conclusion that the bottle contained arsenic, and they both stated that fact in their examination before the magistrates and the coroner. He had used the same description of copper gauze for a great many years, and never before discovered arsenic in it. He should certainly continue to use it, but not with chlorate of potass. Chlorate of potass would act upon the kidneys and carry off any matters contained in them. He could ascribe the death to no other cause than the administration of some irritant poison. The (supposed) finding of the arsenic in the bottle did not have any effect on the opinion he subsequently formed in reference to the case. The moment he discovered the mistake that had been made, he gave information to Sergeant Ballantine. Dr. Odling and Dr. Brande also analysed portions of the contents of the bottle, and they all came to the conclusion that there had been a mistake in the first instance. If no chlorate of potass or other ingredient had been used to carry off the arsenic in a case of poisoning, he should generally expect to find arsenic in the tissues of the body. No arsenic was found in the body of the deceased. In slow arsenical poisoning the vessels of the stomach might be gorged with blood. There were a variety of symptoms attending slow poisoning by arsenic. Sometimes it caused eruptions on the arms and hands, excoriation of the nostrils, and a hacking cough. A clammy sweating would be one of the consequences of antimonial poisoning, unless there was diarrhœa and vomiting. The action of small doses of antimony upon the liver would be to enlarge the liver and cause a deposit of fat on that organ. Dr. Taylor did not think antimony would have the effect of softening the liver.

Dr. Odling gave evidence agreeing with that of Dr. Taylor. Mr. Brande stated that he examined the bottle of chlorate of potass, and found that it did not contain arsenic.

Mr. Serjeant Parry addressed the jury for the defence. He would not deny that the prisoner had violated the moral, if not a higher law, by committing bigamy: but he contended that none of the circumstantial evidence brought forward tended to fix the guilt of murder on him. He believed the will made by the deceased was the entire foundation for the charge. It might be said that this supplied the motive; but, if the other evidence were insufficient to support the charge of wilful murder, the existence of the will ought not to be allowed to operate in any way upon their judgment. It was said that the prisoner had perpetrated the crime to obtain possession of about £1,700 or £1,800; but he would equally have come into possession of it if the deceased had lived; for she had handed over to him the dividend that she received in April. While she lived, also, she was entitled to a life interest in £5,000, and the prisoner would have had the benefit of that also, and therefore he had a much stronger motive for her to live than he had to destroy her. He was certain they would not forget the uniform kindness and affection with which he had been proved to have treated the deceased, although it was suggested on the part of the prosecution that this conduct was merely practised to deceive and to throw off suspicion. The suggestion was that the prisoner was poisoning this lady, but there could be no doubt that he was the first to propose that additional medical assistance should be called in, and everything he did must have been done under the very eyes of several medical men of the greatest skill and experience. The conduct of the prisoner was extraordinary and unaccountable upon the supposition of his guilt; but it was clear, straightforward, and simple, upon the supposition of his innocence. The evidence by which it was sought to make out that a crime had been committed, was nothing but a theory. It was said that the death arose from the administration of some irritant poison, such as arsenic or antimony. Had any arsenic or antimony, or any other poison been shown to have been, either indirectly or directly, in the possession of the prisoner, or to have been within his reach or under his control? As to the stupid theory that the poison had been carried off by chlorate of potass, it was merely the offspring of a fertile brain, to account for what was felt to be an almost overwhelming difficulty, namely, the absence of any poison in the body of the deceased. Had a conviction for murder by poison ever been heard of without some evidence being adduced that the prisoner had poison in his possession, or that he was in a condition to obtain it? The prisoner was taken suddenly into custody; he was searched, and he had no opportunity of concealing the poison if there ever had been any in his possession. Dr. Taylor, when he was examined originally, stated, without the least reservation, that he had discovered arsenic in a vessel that was in the possession of the prisoner. If this had been the fact it would have been almost conclusive evidence, and if the mistake had never been found out, upon that fact alone the prisoner would have gone to the scaffold, probably perfectly innocent. Dr. Taylor no doubt believed that he was right, but was it not clear that he was wrong? and what reliance ought to be placed upon evidence of this description in a case where the life of a fellow creature was at stake? The same test was applied to the evacuation that was applied to the chlorate of potass, and might there not have been some mistake with the one as with the other? In the common grey powder there was frequently antimony; bismuth also frequently contained arsenic; and Dr. Taylor himself admitted that in the sulphate of copper that came from the surgery of Dr. Julius he discovered slight traces of arsenic. The evacuation which was said to have contained arsenic was handed to Dr. Julius by the prisoner himself. Dr. Julius stood in a very peculiar position. He admitted that on the 18th of April he suspected that poison was being administered; and yet, from that time down to the day before the death of the unhappy lady, he took no steps to obtain a nurse, or to put a stop to the wicked proceeding which he believed to be going on.

The following evidence was then called for the defence:—

Dr. B. W. Richardson deposed that he was a doctor of medicine, a licentiate of the College of Physicians, and lecturer on Physiology at Grosvenor Place School. The symptoms of the deceased, and the *post mortem* appearances, in his opinion, were not reconcileable with slow arsenical poisoning. There was an absence of several symptoms which he should expect to find. The inflammation appeared more developed in that portion of the intestines which, in poisoning by arsenic, generally received the least injury. He should expect, in poisoning by arsenic, that arsenic would be found in the tissues of the body. He had administered arsenic in small doses, with chlorate of potass, to a dog; and, after the dog was destroyed, he discovered

arsenic in the liver, lungs, heart, and spleen. He gave eighteen grains of arsenic in sixteen days: he could not say how much he found in the body—there might have been half a grain. He tried experiments upon two other dogs, with antimony and arsenic given alternately, one in conjunction with chlorate of potass, and the other without. One of these dogs died at the expiration of eleven, and the other of twelve days; and in both cases a portion of the poison passed away in the urine. In slow poisoning by antimony, he should expect to find softening of the liver and congestion of the lungs, and some poison in the liver. The liver was generally the great *depôt* for arsenic or antimony. He had seen many cases of acute and ordinary dysentery; and the symptoms exhibited by the deceased more resembled those of the former malady than the result of slow arsenical or antimonial poisoning. He was acquainted with many cases of pregnancy where the patient suffered from vomiting and diarrhœa. From the description that had been given of the deceased lady, he considered she was a very likely subject for dysentery. He had analysed ordinary bismuth, and had discovered arsenic in many instances; the largest quantity was half a grain in an ounce of bismuth. He had prescribed bismuth to a patient; and, after taking it several days, a certain quantity of arsenic passed from him. Antimony would undoubtedly cause sickness, and sometimes purging; and it would also be calculated to produce a sensation of burning heat in the stomach. He did not believe that sulphate of copper was likely to produce such a sensation. Heat in the abdomen, and the other symptoms he had mentioned, were symptoms of irritant poison having been taken, but they were also symptoms of violent diarrhœa; and whenever there was an excess of acid in the stomach, it was likely to produce a burning sensation. If a patient was suffering from violent irritation of the stomach, from any cause, sulphate of copper would be likely to increase that irritation.

Mr. J. E. D. Rodgers, formerly lecturer on Chemistry at the Grosvenor Place School, had given chlorate of potass, as an experiment, with arsenic and antimony; and it had no effect whatever. Chlorate of potass was very much used as a wash for the mouth, to sweeten the breath. Supposing arsenic and antimony to have been administered to any one for five or six weeks, he should certainly expect to find both substances in the tissues of the body, particularly in the liver, and in the kidneys and spleen; and if he did not find any poison, he should believe that none had been administered. He did not believe it possible to find antimony in the blood without also finding it in the liver. He could state positively from his own experience, that nearly all the bismuth that was sold contained arsenic, he should say to the extent of half a grain to an ounce. Grey powder frequently contained antimony, and he should say more than bismuth would contain of arsenic. The condition of the intestines of the deceased was not consistent with slow arsenical poisoning. There was a peculiar appearance of the conjunctiva that he should always look for in slow poisoning by arsenic, which was absent in this case. In slow poisoning by antimony, he should expect to find a clammy sweat all over the body, and a deathy coldness; and that the liver would be softened. Supposing the sixth of a grain of arsenic to be found in an evacuation, he should think that a large portion still remained in the system. If under such circumstances he did not find arsenic, he should be inclined to doubt whether the experiment on the evacuation had been correct. There were means of obtaining copper perfectly pure; and he considered it a most dangerous thing to use copper of any other description, particularly where a man's life was at stake.

Dr. Thudichum deposed that he was a doctor of medicine, and lecturer on Practical and Experimental Chemistry at the Grosvenor Place School; and he was also a pupil of Professor Liebig. He believed that all the symptoms were consistent with a natural death; and, in his opinion, she died of dysentery. The diphtheritic symptoms were not consistent with death from slow arsenical or antimonial poisoning. In such cases, the poison would be found in the tissues. He had found that grey powder contained arsenic and antimony, and a larger portion of the latter than the former. He had also analysed bismuth, and found it contained both arsenic and antimony. In one inttance, in 120 grains of bismuth he found about half a grain of arsenic. He could not say the quantity of grey powder he had analysed. The mercury the deceased took might have produced the aphthous spots in the mouth; but he did not think the arsenic or the antimony had anything to do with her death. He thought that the quantity of arsenic and antimony contained in the bismuth and the grey powder would account for a small portion of arsenic being found in an evacuation.

Dr. F. C. Webb, lecturer on Medical Jurisprudence at the Grosvenor Place School of Medicine, and a physician of the Great Northern Hospital, gave similar evidence to the last witness as to the cause of death; and said it was his opinion, from the symptoms, that the deceased died a natural death. The pregnancy of the deceased was a material ingredient in the case, and it would have accounted for many of the symptoms that presented themselves. He considered the administration of sulphate of copper to the deceased, in her condition of pregnancy, as very improper; and that bismuth ought not to have been given to her, unless it was pure. He considered the deceased died of dysentery, aggravated by her pregnancy; and he had read of many such cases that had altogether baffled medical skill. If he was called in to attend the wife of a medical man, and her husband told him that she was not in the family way, if he found that the medicines he prescribed did not have their proper effect, he should still feel it his duty to make further inquiries to ascertain whether she was pregnant or not. He should not expect that any medicines would check such a disease as that to which he attributed the death of the deceased. He should not have dreamed of poison if he had attended upon the deceased, and had seen all the symptoms that presented themselves. Fifteen thousand persons died of dysentery in this country from the year 1848 to the year 1854.

Dr. G. F. Girdwood deposed that he had had great experience in midwifery, and he had seen many cases in which pregnancy was combined with dysentery, and in one case the dysentery was very severe.

Mr. James Edmunds, surgeon to the H division of police and to the Royal Maternity Charity, said that in his experience purging and vomiting were very common in the early stages of pregnancy, and the former was frequently as violent as almost to exhaust the patient; and he knew of one fatal case of this kind. This patient was a woman, about forty years old; she was married, and had had several children. The illness continued for several weeks, and she frequently complained of burning pain in the stomach. The witness made a *post mortem* examination. The cæcum and the lower intestines were violently inflamed, and there were ulcers as large as a shilling. The patient was in the seventh month of her pregnancy. He administered bismuth, and it relieved the vomiting for the time, but it came on again. Witness was in attendance to give evidence in another case, but he felt it his duty to communicate with the solicitors for the prisoner, and he was then subpœnaed to attend.

Dr. Tyler Smith, physician-accoucheur to St. Mary's Hospital, deposed that he had devoted great attention to midwifery, and was the author of a work upon the subject. Excessive vomiting was very often a symptom in the early stages of pregnancy, and he had known instances in which it had caused death, baffling all medical treatment. In the case of a patient who was suffering from purging and vomiting, he should consider it very important to ascertain whether the patient was pregnant or not; and this, in his opinion, should always be done in such cases. In pregnancy accompanied by purging and vomiting, it was very common for the patient to feel a burning sensation in the stomach, and the mouth would become sore; and he had seen cases where the symptoms had been so similar to those that would have resulted from the administration of poison, that the evacuations of the patient were submitted to examination by a chemist. In cases of excessive vomiting in pregnancy, prussic acid was a most valuable medicine. It was a very common thing in such cases shortly before death that the patient should cease vomiting, and even be able to take food, and this was the case with the celebrated Charlotte Bronte, who died of vomiting in pregnancy. The death, in point of fact, was the result of starvation; under such cases the countenance of the patient always presented a most peculiar appearance. Any irritating medicine would keep up dysentery.

Mr. Pedley, a dentist in George Street, Hanover Square, deposed that in February last Dr. Smethurst applied to him to give him something for foulness of breath, and he mentioned chlorate of potass as an ingredient that might be used with good effect.

Mr. Sergeant Ballantyne then replied for the prosecution. After commenting on the various points in the general evidence to show that they pointed to guilt on the part of the prisoner, he noticed the medical evidence. Cases of slow poisoning were very rare; there were a great many difficulties to contend with in making out such a case, and it could only be done by circumstantial evidence. If it would be required that positive

evidence should be given of the commission of the offence, this terrible crime of secret poisoning would in almost every case go unpunished. With reference to the mistake that had been committed by Dr. Taylor, he said in his opening that this mistake had been committed; but the jury would recollect that it it had been proved by competent persons that the test he made use of was a perfect test under ordinary circumstances, and it was only defeated by the presence of the chlorate of potass. He then called the attention of the jury to the fact of a certain quantity of arsenic being found in one of the evacuations of the deceased, and he said that one of the medical men who had been examined on behalf of the prisoner had admitted that under such circumstances he should have expected that a large quantity of arsenic had been in the body. A good many fanciful theories had been suggested on the part of the defence; and an endeavour was made to account for the presence of the poison by saying that it was contained in the bismuth that was prescribed by Dr. Julius. But the bismuth which came from the surgery of Dr. Julius was proved to be entirely free from arsenic; and in that which had been analysed by the witnesses for the prisoner, the quantity was quite infinitesimal, and could hardly be appreciated.

The Lord Chief Baron then summed up. After going through the general history of the case, he came to the medical testimony. He observed that the whole of the medical men who had been examined, who were all men of experience in their profession, concurred in expressing their opinion that the symptoms exhibited by this unhappy lady were unlike those of any known disease, and were more ascribable to the administration of an irritant poison than to any natural disorder. The counsel for the prisoner had made a great deal of the mistake made by Dr. Taylor, and he had a perfect right to do so. It appeared, however, that it was clearly a mistake; and that it was owing to a new discovery in science that chlorate of potass would dissolve copper. They must, therefore, give to this fact no more effect than it really deserved; and he certainly considered that the learned counsel for the prisoner had no right to ask them to dismiss Dr. Taylor's evidence altogether, in consequence of this mistake. Dr. Taylor had frankly admitted his mistake; and if it really arose from a new fact in science, he thought it would be rather too much to say that upon such a ground Dr. Taylor's evidence should be altogether rejected. His Lordship then proceeded to comment upon the evidence for the prisoner. Many witnesses, he said, were called, and their evidence appeared to be to the effect that according to their opinion and experience the symptoms were not consistent with a case of slow arsenical poisoning, and that a great many symptoms which were almost invariably exhibited in such cases, were absent. A good deal of their testimony also went to show that chlorate of potass would not have the effect of eliminating the arsenic from the system more rapidly. With regard to this latter portion of evidence, after the discovery that no arsenic was contained in the bottle No. 21, it appeared to him that it was of no value whatever, except so far as the learned counsel had a perfect right to use it to attack the evidence of Dr. Taylor. Another portion of the evidence went to show that the deceased might have died of some natural disease; but none of the witnesses went so far as to say that she actually did die of such disease. Those witnesses also expressed their opinion that in death from arsenic or antimony some portions of these substances would be found in the body, and particularly in the liver. All the evidence, however, the jury would recollect, was merely the evidence of scientific men, the result of reading and study; and, although such evidence was undoubtedly receivable, still the jury must consider how far it ought to weigh against that given by witnesses who saw the patient while living, and who had the opportunity of personally observing all the symptoms. One of the witnesses called on behalf of the prisoner for the purpose of casting a doubt upon the existence of poison, admitted that he had not taken the measures that were necessary to arrive at a correct result, and the only reason he gave for this was that he had not the time. It appeared that no answer whatever had been given to the main point in the prosecution—that no medicine whatever had the slightest effect upon the malady under which the deceased was suffering. The medical evidence was, of course, very important; but the jury must, in addition, look at all the other facts in the case, and particularly to the conduct of the prisoner, and his motives for committing the crime. Even supposing that there were no medical testimony at all in the case, they would still have a very grave question to decide with reference to the guilt or innocence of the prisoner.

The jury, after an absence of forty minutes, returned a verdict of "Guilty."

The prisoner, on being asked in the usual form whether he had anything to say why judgment of death should not be passed upon him, addressed the court for about twenty-five minutes. He admitted that he had acted illegally in marrying the deceased; but swore that he was perfectly innocent of having administered anything wrong to her. He said his wife was aware of the attachment between him and Miss Banks, and that it was arranged that she should never trouble him nor make any inquiries after him, and he was to visit her when he pleased. With regard to the letter addressed to his wife that was found in his possession, he had written to her every week since they had been separated. During Miss Banks's illness, he was always anxious to have additional medical advice; and he declared before God, that it was himself who advised that Dr. Todd should be sent for. The doctors deceived him with regard to the state of the lady, and they always said that she would recover. Dr. Todd had no opportunity of examining the state of the lady, for he was not in her bed-room more than four or five minutes, and as to the extraordinary expression of countenance that he has spoken of, it was very likely occasioned by her seeing three men, one of them a strange doctor, in her bed-room. With regard to the finding of half a grain of arsenic in the motion, all he could say was, that she had fifteen motions on that day, and if there was nearly half a grain in that one evacuation, there must have been at least four grains of pure arsenic in her body, and yet none whatever was discovered. It had been urged against him that no one knew what became of the food that was not consumed by the patient. He used to eat what she left. He denied entirely that there was any secrecy or concealment; the bed-room door at both the lodgings was constantly open, and the landlady, or any one else, could see what was going on in the room. Besides this, he used to go out six days out of seven, and he always went two or three miles to post his letters. He declared most solemnly before God, that he never desired to prevent Miss Louisa Banks from seeing her sister, and as to the supposed motive for committing the offence being to get the deceased's money, he could have had that at any time. She gave him her dividend, and she had such confidence and affection for him, that if he had asked her for the money, she would have let him have it immediately. Besides this, he had no want of money. He had £400 in his possession, besides the amount at his banker's, when he was taken into custody. He also wished to state, with regard to his wife, that he had no property with her, and that they had lived together happily a great many years, travelling about and living in Paris, and also at different places in Germany. He had always acted honourably, and paid everybody, and although his conduct had been raked up for thirty years, nothing had been found out against him, and this singular charge would never have been made but for the ill-feeling that Dr. Julius entertained towards him.

The Lord Chief Baron then pronounced sentence of death: upon which the prisoner exclaimed in a firm voice, "I declare Dr. Julius to be my murderer. I declare I am innocent before God."

The trial lasted five days.

TO CORRESPONDENTS.

Communications have been received from:—Mr. E. Daniell; Dr. W. B. Mushet; Dr. E. Waters; Mr. S. H. Evans; Dr. J. H. Goodwin; Mr. W. Gillibrand; Mr. T. Morris; Dr. T. Skinner; Dr. D. Thomson; Dr. G. D. Gibb; Dr. J. G. Croker; Mr. J. Lucas; Messrs. Savory & Moore; Dr. O. Ward; Dr. F. H. Woodforde; Mr. Spens; Dr. Hyde Salter; Mr. T. Holmes; Dr. Davey; Mr. Edward Lister; Mr. A. T. H. Waters; Dr. James Russell; The Registrar of the Medical Council; Mr. Thompson; Mr. William Martin; Mr. W. F. Cleveland; Dr. P. H. Williams; Mr. J. C. Davie; Mr. J. H. Spencer; and Mr. J. C. S. Jennings.

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WINTER SESSION 1859-60.

LECTURES.

The Introductory Lecture will be delivered on Monday, October 3rd, at Three o'Clock, p.m., by Dr. COCKLE.

General Anatomy and Physiology—Dr. Richardson.
Descriptive and Surgical Anatomy—Dr. Halford and Mr. Lawson.
Chemistry—Dr. Thudichum.
Principles and Practice of Medicine—Dr. Cockle.
Principles and Practice of Surgery—Mr. Spencer Wells & Mr. Adams.

FEES. General Fee to all the Lectures required by the Universities of London and St. Andrew's, the Royal College of Surgeons of England, and the Society of Apothecaries, 35 Guineas.

Special Arrangements may be made for any one of the Examining Boards.

Prizes and Honorary Certificates will be awarded for general proficiency at the termination of the Session.

The Microscope is used to illustrate the Lectures and Demonstrations.

The Dissecting Room and Museum of Anatomy are open to the Students during daylight, where their Studies are superintended by the Lecturers on Anatomy and Mr. PITTARD.

The Lecturer on Chemistry has a Private Laboratory, where Students are instructed in Analytical and Physiological Chemistry.

Instruction in Pathological Anatomy is given by the Lecturer on Physiology.

Further information may be obtained at the School, 1, Grosvenor Place; of Dr. RICHARDSON, 12, Hinde Street, Manchester Square, W.; or at the Residences of the different Lecturers.

St. George's Hospital Medical School.

SESSION 1859-60.—The WINTER COURSE of INSTRUCTION will commence on SATURDAY, October 1st, with an INTRODUCTORY ADDRESS by Mr. H. C. JOHNSON, at 2 p.m., at the Hospital.

Physicians—Dr. Page, Dr. Bence Jones, F.R.S., Dr. Pitman, and Dr. Fuller.
Assistant-Physicians—Dr. Barclay and Dr. John W. Ogle.
Obstetric Physician—Dr. Robert Lee, F.R.S.
Surgeons—Mr. Cæsar Hawkins, F.R.S., Mr. Cutler, Mr. Tatum, and Mr. H. C. Johnson.
Assistant-Surgeons—Mr. Prescott Hewett and Mr. George D. Pollock.
Dentist—Mr. Vasey.

Lecturers—Medicine—Dr. Pitman.
" Surgery—Mr. Tatum.
" Anatomy—Mr. Pollock and Mr. Gray.
" Physiology—Mr. A. Johnson.
" Chymistry—Dr. H. M. Noad.

The Hospital contains 350 beds.

Clinical Lectures are given by the Physicians and Surgeons of the Hospital during the Winter and Summer Sessions, and Clinical Instruction is given in the Wards by the Physicians and Surgeons, and on the Diseases peculiar to women by the Obstetric Physician.

A Maternity Department, for the delivery of married lying-in women at their own homes, is established at the Hospital, under the superintendence of the Obstetric Physician.

The Surgeons' Perpetual Pupils are eligible to be Assistant House-Surgeon for Six Months, and House-Surgeon for Twelve Months (without additional Fee), when properly qualified for the Office. Pupils of the Hospital are eligible to the office of Obstetric-Assistant, when duly qualified, at a salary of £100 per annum.

Pupils entering to St. George's Hospital Medical School are free to all Lectures and Hospital Practice necessary for the Examination of the College of Surgeons and Society of Apothecaries, by the payment of FORTY GUINEAS the first year, FORTY GUINEAS the second year, and TWELVE GUINEAS the third year. But Pupils have the option of entering to the different courses of Lectures and Hospital Practice by separate payments.

EXHIBITIONS AND PRIZES.

"The William Brown Exhibition," of Forty Pounds per Annum, tenable for Three Years, may be held by any Pupil, perpetual to the Medical or Surgical Practice, who has commenced his third, but not completed his fourth Winter Session. Pupils entering to the Hospital Medical School in the Session 1859-60 will be entitled to compete for this Exhibition.

A Prize of Twenty Guineas, for general proficiency in Medical Studies, will be offered to Students who enter to the Hospital Medical School for the Session 1859-60. The Examination will take place in July.

Also, for the encouragement of Clinical Study—

A Prize of Twenty Guineas, for Surgeons' Pupils in their second year; a Prize of Twenty Guineas, for Physicians' Pupils in their second year; Sir Benjamin Brodie's Clinical Prize in Surgery; the Thompson Medal; the Lewis Powell Clinical Prize in Medicine; and Sir Charles Clarke's Prize for Good Conduct.

Further information may be obtained from Mr. Pollock, the Treasurer of the School; from any of the Lecturers; or from Mr. Hammerton, the Apothecary of the Hospital.

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